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Cognitive Droughts

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ABSTRACT

Poverty involves both *low income levels* and *high income uncertainty*. Do both these dimensions of being poor capture attention in ways that distort decision-making and trap people in poverty? We examine these issues using real-life shocks faced by farmers in Brazil: random payday variation affecting income levels, and rainfall shocks that affect income uncertainty. We find that it is income uncertainty that systematically has adverse cognitive effects; low income levels affect only the poorest households. The net adverse impacts on cognitive function prevail even though both dimensions of poverty reallocate attention to scarce-resource tasks. These results broaden our understanding of the impacts of uncertainty by exploring a psychological channel distinct from risk aversion, and help reconcile apparently contradictory evidence on the cognitive impact of poverty in previous studies.

JEL Codes: D81, D91, I32

Keywords: Uncertainty; Attention; Psychology of poverty; Scarcity

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1 Introduction

In the Netflix documentary *Living On One Dollar*, four economics majors decide to live in a Guatemalan slum for about two months to experience poverty first-hand. In order to approximate the lives of the poor, each day they draw a paper slip from a bag, which determines how much income they can use for living expenses on that day. The paper slips average 1 dollar, hence the title of the documentary – but not all of them are worth the same. In particular, several paper slips are worth zero. This little experiment captures two fundamental dimensions of poverty: it involves enduring (1) *low income levels* on average, but also (2) *high income uncertainty*, at the risk of not having enough to meet even basic needs on any particular day.[1] This paper studies the extent to which each of these two dimensions drive the psychological effects of poverty on decision-making.

Income uncertainty is a central feature of the lives of the poor in the developing world (e.g. Scott, 1977; Karlan et al., 2014), although not exclusively so. Over the last forty years, income uncertainty has now become a reality for poorer households in rich economies like the US, too.[2] Rational responses to that uncertainty – such as through risk-aversion – have been extensively studied by economists (e.g. Dercon, 2002; Rosenzweig and Binswanger, 2005). However, risk aversion in choices made under these circumstances does not fully capture how pervasive the effects of income uncertainty under poverty can be. When a person is one illness away from being unable to work and feed one's family or one missed rental payment away from eviction, the scepter of such bad shocks loom large even if they never come to pass – with adverse knock-on effects across *all other* life decisions. No doubt, living on low income *levels* itself involves continually making difficult financial tradeoffs between expenses that feel equally important, which can be cognitively and emotionally taxing. Nevertheless, the psychological tax from

[1] Throughout, we use the term *uncertainty* as a stand-in for either *risk* or *known uncertainty* (as in Bloom, 2014), rather than unknown or *Knightian* uncertainty (Knight, 1921).

[2] For instance, Dynan et al. (2012) find that American household incomes became 30 percent more volatile between the early 1970s and the late 2000s – a pattern corroborated by other studies drawing on numerous nationally representative data sets including the Survey of Income and Program Participation (SIPP) and the Current Population Survey (CPS). See Morduch and Schneider (2019), Chapter 1 for details. The findings from both their financial diaries project and the nationally representative Survey of Household Economics and Decision-making (SHED) show that a disproportionate burden of such income uncertainty falls on poor families, making it a hidden source of inequality in the US.

worries induced by income uncertainty may be larger, because of the sense of helplessness and loss of control over one's decisions that come with it.[3] In this paper, we provide first-hand evidence of the differential effects of these two dimensions of poverty on attention allocation and cognition among the poor.

We focus on these specific dimensions of psychological impact because attention allocation is a unifying theme for much of behavioral economics (Gabaix, 2018), and because cognitive function is at the core of all decision-making (Burks et al., 2009; Dohmen et al., 2010; Benjamin et al., 2013). To guide our empirical investigation, we draw on Mullainathan and Shafir (2013), which proposes two possible types of cognitive effects of poverty. First, worries that preoccupy a poor person could deplete (or tax) her overall mental attention resources. In the cognitive psychology literature, this effect is referred to as *cognitive load*. Second, a poor person's available attention resources can be captured by (or reallocated to) the urgent or imminent challenge where scarcity looms. This effect is referred to as *tunneling*. Together, these two effects could affect the quality of decisions of the poor, by reducing attention resources available for important issues or by diverting them too narrowly to issues that are urgent. We provide multiple field-based measures of both these conceptually-grounded aspects of attention allocation under poverty.

To study the impact of high *income uncertainty* under poverty, we exploit daily natural rainfall variation during the rainy season. We combine these shocks with lab-in-the-field survey experiments that randomly expose some farmers to drought-related worries (what the cognitive psychology literature calls priming). This two-pronged exogenous variation approach to studying uncertainty allows us to combine the strengths of both approaches.[4] To examine the impact of low *income levels*, the natural shock we use is variation in the timing of payments under Bolsa Família, Brazil's flagship conditional cash transfer (CCT) program. Under this program, a beneficiary's monthly payday is determined by the last digit of her social security identification number (Número de Identificação Social, or NIS), which is randomly assigned. Hence, distance

[3] For instance, the findings of the 2014 Pew Charitable Trust survey are consistent with such a more damaging psychological impact of income uncertainty: when asked if they would prefer to be a little richer or have a steadier, more stable financial life, 92% of American respondents chose stability over mobility (Morduch and Schneider, 2019).

[4] Rainfall shocks provide external validity because they occur naturally in farmers' daily lives, varying at the municipality-survey level. Survey experiments shed light on the specific psychological mechanism of interest, hence contributing to internal validity; they also improve precision because they vary at the individual-survey level.

to payday at the time of our surveys is as good as random. Since the program has been in place for over ten years, beneficiaries in our sample know exactly what they will be paid every month and when. Thus, the only difference between a subject who has already been paid and another who still has not at the time of our surveys is in their income *levels*; there is no difference whatsoever in uncertainty.

There could be some concern that daily rainfall shocks could affect both income uncertainty *and* the income level itself. Having said that, the pattern of worries and cognitive function we observe in response to these shocks points to distinct effects of uncertainty.[5] Furthermore, given that there is no uncertainty in the Bolsa Família payment (in timing or amount), any difference in the effects of these two sets of shocks could be attributed to the effect of uncertainty alone.[6]

To separate the cognitive effects of low income levels and high income uncertainty, we study the impacts of these mutually orthogonal shocks described above *within the same setting* and *time frame*. Our study uses a sample with substantial spatial and temporal variation to examine these effects: 47 municipalities in the drought-prone State of Ceará, in Northeast Brazil, where we track the behaviour of 2,800 farmers over the course of the rainy season. We introduce multiple innovations to overcome the logistical challenges of studying subtle psychological effects over such a wide field (in space and time). We survey farmers over automated phone calls (IVR), adapting standard (visual) attention tests to audio in order to gather reliable data on attention and cognition in response to shocks across space and over time. Farmers do not pay to participate, and are incentivized by airtime credit. Cognitive function is captured by participants' overall performance in tasks that measure their working memory, attention and impulse control. Attention reallocation is captured by participants' relative performance in tasks involving scarce resources (money and water) and by their stated valuation of scarce resources in trade-offs against other, non-scarce resources.

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[5] We find that two specific predictors of worries (picked using LASSO), rainfall 3 days and 7 days prior to *each* survey, have large cognitive effects. Even though both variables should not have systematically different effects on income levels, we find a *larger* cognitive effect of rainfall 3 days before relative to no rainfall 7 days before the survey. This finding is consistent with rainfall shocks making uncertainty top of mind. See sections 3.2.1 for details.

[6] We note that in our sample, the magnitude of the *Bolsa Família* income shock is similar to that of the expected impact of the rainfall shock on harvest income. We note that priming is designed to affect respondents' sense of income uncertainty alone.

We have two key findings that we describe below. First, income uncertainty (through negative rainfall shocks or priming) induces significant cognitive load. The drop in performance in tasks measuring cognitive function is equivalent to losing about 25% of one's harvest at the end of the rainy season or to downgrading a farmer from high school back to elementary school (in a cross-sectional comparison). These magnitudes suggest that this hitherto unexamined *psychological* impact of income uncertainty could have sizable implications for decision-making among the poor. These adverse cognitive load effects peak at the low-end of the income distribution, and gradually taper off in richer municipalities. Such income uncertainty also induces tunneling: respondents are better and faster at finding words related to scarce resources, likely to value such resources more highly in trade-offs against non-scarce resources, and less sensitive to framing in tasks involving these resources when exposed to priming and bad rainfall shocks. Further, being primed with worries in the face of bad rainfall shocks compounds such tunneling effects.

Unlike income uncertainty, low income *levels* induced by random payday variation *do not* systematically increase cognitive load, except *within the poorest municipalities*; in those areas, the magnitude of cognitive load induced by being a week away from payday is large – even higher than that due to income uncertainty.[7] Low income levels also induce a tunneling effect, both within these poorest municipalities and across the rest of the income spectrum.

Given such large attention reallocation towards scarce resources across both types of shocks, could it be the case that, in the real-world tasks that the poor engage in under financial pressure, such tunneling overturns the adverse cognitive load effects of being poor? The effect sizes of each mechanism across the income distribution suggest otherwise. Using standardized measures for our cognitive load and tunneling outcomes, we find that it is the cognitive load effect that dominates overall. Taken together, our results show that the cognitive burden imposed by income uncertainty makes farmers 'penny wise and pound foolish'.

Our final insight is about how these two sets of findings allow us to reconcile apparently contradictory results across previous studies on the cognitive impacts of poverty. Field evidence

[7] Such load is fully consistent with the fact that conditional cash transfer payments are a much larger share of family income in these areas, creating significant stresses from managing difficult financial tradeoffs on a tight budget as the next payday approaches.

from sugarcane farmers around harvest (Mani et al., 2013) and workers around payday (Kaur et al., 2019) as well as lab-in-the-field evidence from the US (Shah, Mullainathan and Shafir, 2012, 2015) shows significant cognitive load from poverty. In sharp contrast, a recent study of low-income US households does not find evidence of adverse cognitive effects (Carvalho, Meier and Wang, 2016). Our results offer a simple explanation for this seemingly contradicting evidence: farmers in the sugarcane study faced uncertainty in the amount and/or the timing of their harvest payments; conversely, this is unlikely for respondents in the Carvalho, Meier and Wang (2016) study, since only respondents who provided complete information about the number of payments and payment dates during the study period were included.[8] As we show, it is income uncertainty under poverty that systematically depletes mental attention resources of the poor; low income levels create cognitive load effects only among the poorest of the poor. Thus, the absence of cognitive load effects in the US study by Carvalho, Meier and Wang (2016) could be explained by (i) the lack of uncertainty in payment timing and amounts, coupled with (ii) higher income levels of the US respondents, relative to Indian farmers in Mani et al. (2013) and to the poorest Brazilian farmers in the present study.

To summarize, our findings show that income uncertainty lies at the core of poverty's psychological tax. This result contributes to a burgeoning literature on the economic effects of uncertainty (Bloom, 2014; Bianchi, Kung and Tsikhi, 2018; Bloom et al., 2018). For instance, Bloom et al. (2018) models recessions as (productivity) shocks with a negative first moment and a positive second moment – which, in the terminology we use here, translate to adverse effects of lower income levels and higher income uncertainty, respectively. Our results suggest that uncertainty that characterizes recessions can impair decision-making at the micro level, especially among the poor. Such cognitive responses could, in turn, magnify the effects of such macro-level shocks. Also, the fact that cognitive effects of uncertainty are worse at lower income levels distinguishes the mechanism we highlight from the loss of the 'power of certainty', which has been shown to impair contingent reasoning irrespective of income levels (Martínez-Marquina, Niederle and Vespa, 2019).

[8] See footnote 13 of Carvalho, Meier and Wang (2016).

Our findings also link to an emerging literature on the psychology of poverty and decision-making. Apart from the aforementioned work, this literature explores the impact of psychological channels such as stress (Haushofer and Shapiro, 2016) and conditions associated with poverty, such as alcohol consumption, pain, sleep deprivation, environmental noise and malnourishment, and on decision-making (e.g. Schilbach, 2019; Bessone et al., 2019; Dean, 2019; Schofield, 2014). It also ties into a larger literature on behavioral development economics, which examines how biases coming from non-standard preferences, beliefs and decision-making could explain many puzzles in developing economies (see Kremer, Rao and Schilbach, 2018, for a comprehensive survey).

Lastly, given uncertainty's potentially costly consequences for decision-making among the poor, our findings suggest new roles for existing policy instruments – such as providing insurance against income uncertainty (Lichand and Mani, 2018) – and reasons why those that boost income levels alone (such as cash transfers) may not be fully effective.[9] They also point to a need to combine those with new instruments, specifically designed to counteract detrimental effects of inefficient attention reallocation; in particular, adapting the choice architecture – e.g. by increasing the salience of relevant decision features, as in Lichand et al. (2019) – has the potential to enhance decision-making among the poor.

2. On the Ground: Study Setting, Sample and Survey Implementation Logistics

2.1 Setting: The State of Ceará, Brazil

Ceará is a poor and drought-prone State in Northeast Brazil. Over 80% of its territory lies in the semiarid region, and about 60% of its municipalities experienced below-normal rainfall levels (among the bottom 1/3 rainfall levels in the previous 30 years) at some point in the 4 years prior to the experiment. In an extreme year such as 2013, all municipalities except the State capital, Fortaleza, declared a state of emergency, necessitating emergency funds from the federal government to support the estimated 1.8 million family farmers living in the State. Irrigation and modern agriculture techniques such as drip irrigation are rare in the state, and most farmers have

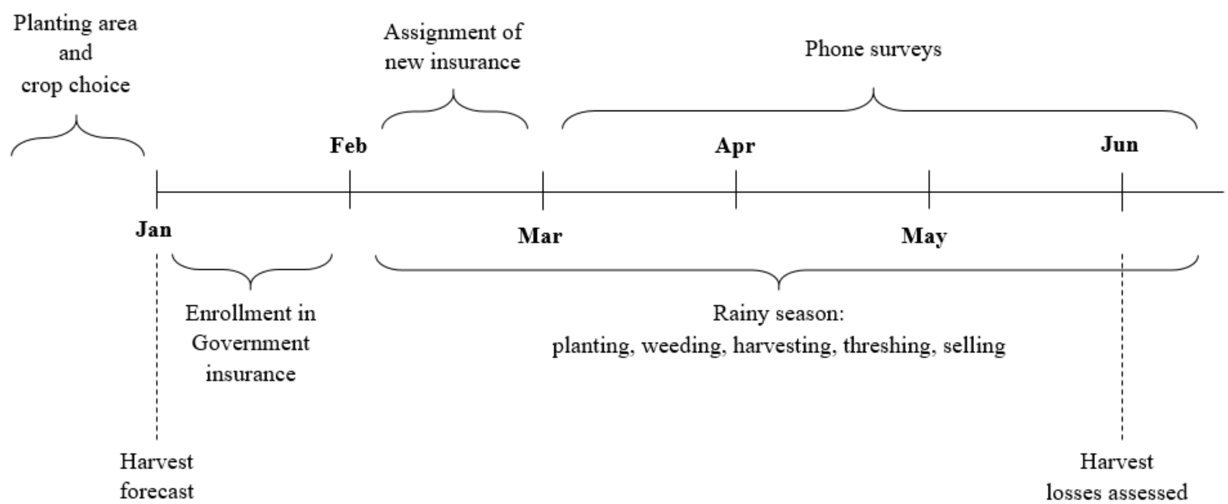
[9] Bloom et al. (2018) also show that increased uncertainty makes 'first-moment' policies, like wage subsidies, temporarily less effective because firms become less responsive to price changes in the face of uncertainty.

to rely solely on rainfall.[10] This setting generates a great deal of anxiety and mysticism linked to rainfall forecasts (see Taddei, 2013, for a detailed anthropological account), making it an appropriate environment in which to study the psychological effects of uncertainty.

2.2 Timeline

The rainy season in most of Ceará spans February through May. In exceptionally good years, the southern part of the state might also have rain in the pre-season, i.e. in December and January, and the state as a whole might have rain in the post-season, i.e. in June and July. Most production decisions – in particular, land preparation and crop choice – are undertaken before January. If rainfall allows, most farmers plant corn and beans only, while a small share of them also plants cassava. Over the course of the rainy season, the margin that farmers can adjust on involves labor. Enrollment in government index insurance generally takes place by the end of January, before the onset of the rainy season.[11]

Timeline



[10] This pattern is seen in the rest of the world too: over half a billion people worldwide live in arid regions without access to irrigation. A substantial share of this population is made of farmers, and the rural poor living in drought-prone areas outnumber those living in favored areas by a factor of two (Barbier, 2010). In Africa alone, droughts affect between 40 and 70 million people every 5 years. The economic costs of these events are high, and they rise almost one-to-one with the share of agriculture in GDP (Benson and Clay, 1998). See also the World Bank's *World Development Report 2014* on "Risk and Opportunity," for a description of the huge variety of macro and micro risks faced by households and firms in developing countries.

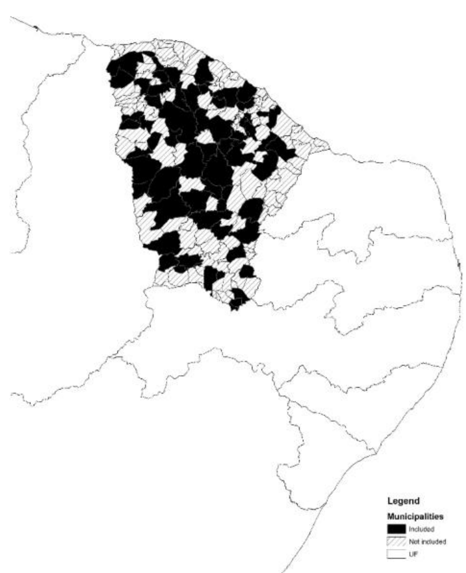
[11] See Lichand and Mani (2018). In that companion paper, we document that index insurance does not systematically mitigate farmers' worries and cognitive load.

Our baseline data collection was carried out in the month of February, after farmers had made planting decisions. This timing is in keeping with our interest in measuring the psychological effects of income uncertainty on decision-making through channels other than risk aversion (for instance, through crop choice). Subsequent waves of data collection were planned for the first two weeks of each of the following four months, from March through June.

2.3 Data Collection Sample

In partnership with Ceará's Rural Development Secretariat, we enrolled 4,084 farmers across 47 municipalities of the hinterlands of the state in our study, in January 2015. In each of these municipalities, enrolment was carried out through agricultural extension workers, who were given 100 consent forms to be handed to the family farmers they oversee. Farmers who opted-into the study provided their mobile phone number to us through these forms. Within each municipality, we directed half of the forms to farmers living in the most drought-prone region in the municipality, and the other half to those living in the least drought-prone region. Due to the high heterogeneity in microclimates within-municipality, we use this information for stratifying treatment assignment in the survey experiment.

Geographic coverage of the surveys



Of the 4,084 farmers enrolled, 2,822 farmers responded to one or more of our survey phone calls over the 4 waves of data collection. Each wave consisted of 6 phone calls, resulting in a total of 24 phone calls. Table C1 shows the distribution of respondents for each number of calls, from 1 to 24. As seen there, about 50% of the sample took up to 8 calls, while 1,262 farmers responded to none. About the latter group, we cannot tell if they did not respond because the phone number provided was wrong or inactive at the time of the surveys, if the telecommunications' tower coverage in some regions was poor enough that they never had a connection when we placed the calls, or if they changed their minds and were no longer interested in participating. Appendix C presents detailed balance and selective non-response tests.

2.4 Cross-Matching with Bolsa Família Payday Data

In addition to our own data collection, we obtained data for a subset of our respondents on their family's monthly payday under Bolsa Familia (Brazil's flagship conditional cash transfer program, in place since 2003), as follows. First, we linked a family's farmer ID (*Declaração de Aptidão ao PRONAF*, DAP) to his or her unique individual social information number (*Número de Informação Social*, NIS) (achieved for 96.4% of our respondents). Next, for every successful match, we obtained information from CadÚnico, the administrative cadaster for Bolsa Família, to verify whether that household was actually receiving CCT payments at that time.

Payday for each matched household depends on the last digit of NIS for Bolsa Família's *main beneficiary*. For this reason, we cannot assign payday simply based on the NIS of our matched subjects, as someone else in the household (e.g. the spouse or an elderly household member) could be the one on which the payment schedule is based. Even though CadÚnico lists all NISs for each matched household in our sample (that of the main beneficiary, and that of the alternate, if available), we do not have information on which one among those is the NIS of the main beneficiary.

To make this assignment, we resort to the following procedure: whenever there is only one NIS in the household, assignment is straightforward. However, if there are two, we apply an algorithm to identify female Brazilian first names[12], as women are primarily the main beneficiaries of Bolsa Família (for 92% of the households, see Bartholo, 2016). Finally, if the algorithm identifies either none or both first names as female, our assignment algorithm picks the first NIS listed for that household in CadÚnico as the main beneficiary. Doing so yields 1,035 subjects in households receiving Bolsa Família with information about their conditional cash transfer payday (36.7% of our sample). While this might appear to be a small sample, we note that the variation within this sub-sample of respondents across 24 surveys (4 waves, with 6 calls within wave) is substantial enough to generate power to precisely estimate cognitive effects.

Distance to payday varies by call and month (exact dates are shown in Table C3). Payments always take place in the last 2 weeks of the month (other than weekends and holidays). The exact dates of our phone surveys varied a bit month-by-month for logistical reasons. Table C4 presents the distribution of distance to payday in our sample, by survey wave.[13]

Taking all waves together, 33.2% of observations within the Bolsa Família sample are within a week of payday (either before or after). Density is enough to allow us to detect effects sizes similar to those we document for priming and rainfall shocks in the full sample, within reasonable time windows from payday.

2.5 Survey Implementation Logistics: The Use of Lab-in-the-field Technology

While it would be ideal to measure psychological outcomes in a controlled lab environment, achieving this ideal would be prohibitively costly in our setting. Research infrastructure is spatially concentrated, while subjects are scattered across many locations – some of them more than 5 hours away from the state capital.

Equally, conducting 24 rounds of lab-in-the-field experiments across almost 50 different towns in the hinterlands of Brazil poses a non-trivial logistical challenge. We therefore take advantage of the fact that almost all poor Brazilian households have access to cell phones, to run such

[12] https://github.com/meirelesff/genderBR

[13] Figure 4 shows that cognitive effects (if any) are concentrated in the few days closer to payday. Hence, the classification of observations sufficiently far from payday as before or after does not drive our results.

experiments via phone surveys (interactive voice response units, or IVR).[14] Farmers receive automated voice calls (computer-managed surveys narrated by a human voice), and respond to incentivized numerical and categorical questions through keystrokes on their cell phones.

Running lab-in-the-field experiments over the phone allows us to measure the outcomes of interest, but it also entails three challenges. First, while we have to measure a number of outcomes in order to estimate the effects of each treatment on both cognitive load and tunneling, attrition in phone surveys can be high, particularly for longer calls. To deal with that issue, we divide our lab experiments into 6 calls of at most 5 minutes each, spread over the course of 2 weeks within each wave. Second, many known psychological tests used to measure cognitive functions, such as stroop or word search, involve visual elements that must be adapted in a manner suitable to conducting them over the phone. To deal with that issue, we design audio versions of stroop and word search (to our knowledge, this is the first paper to perform audio versions of these tests).[15] Third, farmers may have no interest in taking these psychological tests seriously, a possibility that could greatly limit the statistical power of the tests we undertake. To deal with this issue, we grant farmers credit for the time they spent responding to our calls, and incentivize performance in cognitive tests, offering an extra top-up in airtime credit of USD 0.50 for the 20% top-performers in each wave.[16]

3 Measuring the Psychological Impact of Low and Uncertain Incomes: Concepts, Experimental Design and Estimation

Section 3.1 offers a conceptual framework that describes existing theories on how low income levels and high income uncertainty can affect attention resources and decision-making among the poor. Subsection 3.2 describes the study design, including the sources of variation – some experimental and others naturally occurring – that we exploit to examine these psychological effects. Next, subsection 3.3 describes the main outcome measures we use to capture these effects.

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[14] Over 90% of Brazilian households had access to mobile phones already in 2015 according to the Brazilian Institute of Geography and Statistics (IBGE)'s National Household Survey.

[15] These new tests were validated in the field through face-to-face surveys; results are shown in the Supplementary Appendix.

[16] Incentives were designed to be powerful enough to make sure subjects answer calls and take the tests seriously. The expected hourly wage from taking all surveys is USD 3.25, about four-fold the average hourly wage reported by our sample.

Finally, subsection 3.4 gives details on the econometric procedures we use to estimate the effects of interest, address selective attrition challenges, and account for how we handle multiple hypotheses testing.

3.1 Conceptual Framework

Living on a low level of income without access to opportunities to smooth consumption implies that the poor find it harder to anticipate and cope with even known uncertainties associated with their income. As a result, the threat of a one-time shock that could derail a poor family and drag it into a downward spiral could take a huge psychological toll, over and above the toll from having to manage on a meagre income.[17] We hypothesize that such psychological effects would arise from exposure to risk itself – whether or not it eventually materializes.[18]

Psychological theories consider a variety of mechanisms – other than risk aversion – through which such income uncertainty may affect decision-making. One is *anticipation and dread* (Elster and Loewenstein, 1992), formalized by Caplin and Leahy (2001). According to this theory, an anxiety parameter directly enters the utility function due to exposure to future risk, penalizing present consumption experiences. It could also affect expected utility, with a knock-on effect on decision making.

An alternative mechanism is the affect heuristic (Finucane et. al, 2000). According to this theory, feelings influence how individuals *perceive the probability distribution of future states* and, hence, the expected outcomes of such a lottery. For instance, a previous negative experience could mean that a person perceives the probability of the bad state to be higher than it actually is. Related to this mechanism, there is a literature on the effects of trauma (Callen et al., 2014;

[17] For instances of such threatening one-time shocks, see Banerjee and Duflo (2011, Chapter 3) on poverty traps created by a one-time health shock – a cycle of lost income, greater health expenditures and mounting debts. In the US context, see Desmond (2015) for a searing ethnographic account of the consequences of housing evictions among the poor, following a missed house rent payment: worse prospects for future housing and jobs, children pulled out of school, and moving to neighborhoods with higher rates of crime. Studies have linked eviction to psychological trauma (Fullilove, 2005) and have identified it as a risk factor for suicide (Serby et al., 2006).

[18] Inattention due to preoccupation with such worries is consistent with evidence which shows that the poor are less productive workers (Kim et al., 2006) and less attentive parents (McLoyd ,1998). Fewer parent conversations with children has been linked to a vocabulary gap of 15 million or more words between children from poorer versus richer households by the age of four (Fernald, 2013).

Malmendier and Nagel, 2011) which links the effects of past shocks to those of future risk through emotional states (Lerner et al., 2014).

Other theories posit that exposure to uncertainty may result in psychological effects that cause people to move away from optimizing behavior. For instance, the "risk as feelings" hypothesis (Loewenstein et al., 2001) posits that exposure to risk may lead individuals to deviate from the maximization problem entirely, with decision-making dominated by the emotional states elicited by the presence of risk. In this latter case, risk could deteriorate the quality of *all decisions,* not only those related to consumption smoothing.

Finally, the scarcity hypothesis (Mullainathan and Shafir, 2013) posits that individuals worrying about scarce resources suffer two types of consequences. First, such worries deplete mental resources (or bandwidth), hence reducing what remains available to be gainfully harnessed, i.e. they induce *cognitive load*. This effect predicts lower attention and memory, and increased susceptibility to biases. Second, worries make scarce resource challenges top-of-mind, hence they result in mental resources being more narrowly diverted to scarce-resource tasks, i.e. they induce *tunneling*. This theory differs from the previous ones inasmuch as it predicts not just an overall deterioration in quality of decisions and task performance – but also better *relative performance* in specific decisions and tasks – those involving scarce resources.[19,20]

While this scarcity mechanism has been tested specifically for those facing low income *levels*, we conjecture that such a mechanism should extend to the high income uncertainty associated with poverty. Given the more precise predictions of the scarcity hypothesis, we measure the psychological effects of risk and uncertainty using their concepts of *cognitive load* and *tunnelling*.

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[19] See Schilbach, Schofield and Mullainathan (2016) and Dean, Schilbach and Schofield (2019) for further discussions related to poverty and cognitive function.

[20] While these effects could be rational responses to changes in the relative price of allocating attention to different tasks, lab experimental evidence is also consistent with an element of *unconscious* (or irrational) 'mental capture'. For instance, tunneling responses triggered by particular stimuli occur at speeds below the consciousness threshold (Mullainathan and Shafir, 2013). Such unconscious mental responses are consistent with the idea of 'System 1' thinking described by Kahnemann (2011) or the first of a two-stage optimization process as modeled under the Sparsity framework (Gabaix, 2014).

3.2 Study Design

Our study design uses a combination of lab-in-the-field experiments, naturally occurring rainfall shocks and administrative data that creates rich, exogenous variation in the income uncertainty and income levels of our study respondents – which allows us to take a causal interpretation of the cognitive impacts we observe in our data.

3.2.1 Variation in Income Uncertainty: Survey Experiments and Rainfall Shocks

The ideal experiment to study the psychological responses to uncertainty and risk would randomize the allocation of risk. Furthermore, to nail whether the effects of interest are concentrated among the poor, it would stratify random assignment of such risk by income levels. Practically speaking though, it is not possible to randomly assign farmers different degrees of rainfall risk.

However, given the psychological channel of interest to us, it is possible to randomize *worries* about income uncertainty, in the spirit of mechanism experiments (Ludwig, Kling and Mullainathan, 2012). We approximate the ideal experiment through survey experiments that make some farmers, but not others, worry about the possibility of droughts within each survey (a technique that the cognitive psychology literature calls *priming*). The advantage of this approach is control: the variation is randomly assigned at the individual level, and precisely linked to the mechanism of interest.

Taking advantage of the IVR technology, we prime subjects at the beginning of each survey. Upon consenting to take a call, each farmer is randomly assigned to answer a question, either about droughts (treatment group) or about soap operas (control group). The idea is that soap operas are interesting enough that people do not hang up, but that they should not make one systematically worry about rainfall. We note that this random assignment is at the level of each survey *call*, rather than across individual respondents. We also vary the specific wording of this opening question across calls in order to sustain greater participant interest and responsiveness across surveys. Apart from this opening question, the others that follow are identical for the treatment and control groups within each call.

As a second source of variation in uncertainty, we also exploit rainfall shocks using daily rainfall data. These shocks occur naturally in farmers' environments, which increases the external validity of our findings about their impact. Unlike with the survey priming experiments described above, this variation is captured at the municipality-call level rather than at the individual-call level.

Given the availability of multiple rainfall shock measures to choose from, we resort to a data-driven selection procedure based on their ability to predict *drought-related worries*, as follows. We regress worries about rainfall on 51 distinct measures of rainfall over the course of the last 30 days in each municipality, from the occurrence and levels of rainfall at different days prior to each survey, to cumulative rainfall and deviations from historical averages (see Appendix A). We include municipality fixed effects to net out variation linked to systematic characteristics in local climate that are not randomly assigned. All rainfall variables derive from Ceará's official monthly rainfall data, collected by local meteorological stations for each municipality over the past 30 years.[21]

Using LASSO to trade-off goodness-of-fit against over-fitting, our algorithm picks nine variables most predictive of *worries* about rainfall. We then build a post-LASSO summary measure of negative rainfall shocks (which we call the *No-rainfall summary measure*), weighting each predictor by its coefficient in the LASSO regression (see Table C2 for the list of variables picked by the algorithm).

A possible concern with rainfall shocks is that they may affect not only income uncertainty, but also the *level* of income itself. To address this concern, we highlight the effects of two specific predictors of worries separately: no occurrence of rainfall *3 days before* and *7 days before* each survey. While rainfall over a whole planting season naturally affects income levels, it is unlikely that rainfall specifically 3 days or 7 days before any particular survey will do so, or that effect sizes should vary between these two variables.[22] Further, even if rainfall shocks did affect both

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[21] When there is more than one meteorological station within a municipality, the state also reports the average rainfall level for the municipality as a whole. Since we do not have the GPS location of the farmers in our sample, we do not explore information at finer aggregation levels.

[22] In fact, we find that the effect sizes of no rainfall 3 days before the survey on cognitive function are *larger* than those of no rainfall 7 days before, even though whether it rained in one or the other day (just 4 days apart) should not

income uncertainty and levels, any difference in their impact relative to the Bolsa Família income level shock (described below) must be attributed to the income uncertainty component given the similar magnitudes of those shocks (see section 3.2.2). To meaningfully compare the magnitude of the impacts of these two types of shocks, we define direct counterparts to the rainfall shocks 3 days and 7 days before survey, in the payday time window (see the next section).

Last, we can take advantage of these two specific rainfall shock variables to help address the concern of selective non-response in our phone surveys, using Lee (2009) bounds (see section 5.3).

3.2.2 Variation in Income Levels: Before versus After Bolsa Família Payments

Finally, to study the impact of changes in income level, we exploit random variation in farmers' Bolsa Família payday. The schedule for monthly payments is assigned based on the last digit of the NIS – which is randomly assigned – and is publicly available at the Ministry of Social Development website. Staggered paydays are in place to avoid over-crowding at banks and other cash collection points at the time of payment (Kaufmann, La Ferrara and Brollo, 2012). Importantly, since the program had been in place for over 10 years at the time of the experiment, and individual paydays are public knowledge, there is *no uncertainty* about the timing of payment, or the likelihood of receiving it.[23]

The random assignment of payday coupled with the fact that we control the time at which respondents take the phone surveys in our study, makes the *distance to payday* at the time of their response as good as random.[24] We capture the effects of income level shocks by comparing the outcomes of respondents *before* versus *after* payday. We do so using three different measures of *symmetric* time windows around individual paydays: an indicator variables of payday within 3

systematically matter for one's harvest several months down the line. This finding is consistent with rainfall shocks making uncertainty top-of-mind, which is the channel of interest here.

[23] There are no other significant sources of income for respondents in our sample, apart from harvest income and Bolsa Família payments.

[24] In contrast, respondents in Carvalho, Meier and Wang (2016) could choose when they started and completed their online follow-up survey within the 7-9 day window around payday. These timing choices are potentially endogenous to financial pressure, which could vary by distance to/from payday for those receiving paychecks in short time intervals. If so, a study design that allows participants with high frequency paycheck to choose time of response may not be suited to accurately measure the cognitive impact of poverty. See Mani et al (2019) for a discussion on this.

days, within 7 days (as mentioned earlier) as well as a linear measure of distance to payday (ranging from -15 to 15; see Table C4).

In terms of the relative magnitude of rainfall versus payday-related shocks, the size of the Bolsa Família shock is large. Since Bolsa Família's monthly payments at the time were about a third of the typical market value of a family farmer's harvest in the region, and since average harvest losses over the previous 5 years were about a third of expected output too, it turns out that, in our study, in any particular wave, these conditional cash transfer (CCT) payments have the same expected value as a farmer's harvest.

Finally, we can benchmark cognitive effects to those of municipality-level harvest losses. Harvest losses are measured by Government as the difference between *estimated harvest*, based on projections for planting area and yield in January (pre-season), and *actual harvest*, verified in late May (post-season) through audits in randomly selected plots in each municipality. Since the January predictions account for all information available before the rainy season (including planting area and crop choices), harvest losses can be considered randomly assigned.[25]

We also estimate heterogeneous treatment effects of these shocks by income at the municipality level, using per capita income data from the Brazilian Institute of Geography and Statistics (IBGE) 2010 Census. This allows us to gauge whether the effects of interest are concentrated among the poor.

3.3 Measuring Outcomes

Following the conceptual framework outlined in Mullainathan and Shafir (2013) described in section 3.1 above, we measure cognitive outcomes in two categories: *cognitive load* and *tunneling*.[26] In this sub-section, we offer a description of the questions and tasks used for measurement, as well as the underlying rationale for how we interpret each outcome measure. We refer the reader to Appendix A for further details on the specific questions used for these measures.

[25] Given the way harvest losses are computed, they only vary at the municipality level.

[26] We have pre-registered the study at AEA Social Sciences RCT Registry, specifying how different outcomes would be grouped into cognitive load and tunneling (which we also refer to as *focus* in the pre-analysis plan).

3.3.1 Cognitive Load Measures

Cognitive load includes tasks aimed at assessing executive functions (working memory, attention and impulse control; Diamond, 2013) as well as subjects' sensitivity to anchoring (a cognitive bias defined as the tendency to rely on an irrelevant initial piece of information to make subsequent judgements; Kahneman, 2011).

The motivation for looking at this range of executive functions is that they constitute the foundations of decision-making; attention, memory and impulse control should have pervasive effects across the different domains of farmers' decision-making. The motivation for looking at anchoring is to check whether respondents prioritize the right information for their decisions, or whether their attention is distracted by irrelevant details. For instance, our pilot data show that farmers' production decisions show anchoring effects as they try to anticipate future prices with past prices as reference.

We measure working memory through digit span tests, in which subjects must recall the correct sequence of digits in a number string (the longer the accurate sub-string recalled, the higher their score). We measure attention and impulse control through stroop tests, in which subjects must answer the number of times they heard a particular digit repeated in a sequence. While it is tempting to press the digit just heard multiple times, the correct answer is (often) not the digit itself. We validate the audio versions of digit span and stroop we created to be ran over IVR using face-to-face surveys, which draw upon the typical tests used in the literature, adapted from Mani et al. (2013); see the Supplementary Appendix I.

For sensitivity to anchoring, subjects are initially primed with a high number (the price per kg of a live goat in the previous year, which was R$ 4), and are then asked to choose a price band for the price of another item (either the future price of beans in their municipality, or the price of a subway ticket in a different location). In this context, we define anchoring as the tendency to choose higher price bands after hearing a higher initial price for an unrelated item.[27]

[27] Price bands were: "below R$ 3.40", "between R$ 3.40 and 3.80", "between R$ 3.80 and 4.20 " and "above R$ 4.20" (see Appendix A).

3.3.2 Tunneling Measures

In principle, worse performance in psychological tests could be entirely due to biological responses from lack of sleep or undernutrition (except in the case of priming – since survey experiments take place always within 5 minutes of the experimental tasks). Tunneling has the potential to help us understand whether attention reallocation is at play.

To test for tunneling, we assess executive function for decisions and tasks involving scarce resources vs. not. In each task, we measure tunneling by computing subject's *differential performance* when the task involves *scarce* resources (water and money, in our setting) relative to when it involves *non-scarce* resources (such as time). If performance changes differentially for the same task or decision "inside the scarcity tunnel", that would provide evidence that the effects are driven by reallocation of mental bandwidth rather than overall depletion alone.[28] Also, tunneling measures offer, presumably, a cleaner way to capture attention effects under poverty using incentivized tests, relative to cognitive load measures.[29]

The tasks we use to measure tunneling include (i) relative valuation of scarce resources in simple trade-offs, (ii) performance in word search games when it comes to finding words related to scarce resources relative to neutral words, and (iii) sensitivity to framing in trade-offs between scarce resources and non-scarce resources (namely, time), based on Shah, Shafir and Mullainathan (2015). In these tasks, a higher tunneling score comes from better *relative* performance in scarce versus non-scarce resource tasks or decisions, i.e. higher relative valuation of scarce resources, better performance in finding words linked to scarce resources rather than neutral words, and lower sensitivity to framing in decisions involving the scarce resource relative to non-scarce resources, respectively. The details are as described below.

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[28] For instance, Shah, Shafir and Mullainathan (2015) document that worries with scarcity (induce through priming) lead to *lower* sensitivity to framing in decisions involving the scarce resource.

[29] Cognitive load predicts worse performance on a task, but incentives would predict better performance, which makes the overall performance impact on an incentivized task ambiguous. This is seen in incentivized tests offered in several recent papers: the 'Wheel of Fortune' self-replication in Shah, Shafir and Mullainathan (2019) (where primed subjects earn more money in the incentivized experiments), Lichand et al. (2019) (where primed subjects earn more money in short-term incentivized attention and memory tasks), and Kaur et al. (2019) (where workers primed about financial strain increase productivity in peace-meal payment tasks). In contrast, such effect of incentives should make it *even easier* to detect tunnelling since it should lead to *better* relative performance on tasks involving scarce resources.

For the first task, we examine the relative valuation of scarce resources in simple trade-offs – between money and cashews, or between water and cashews – relative to the valuation of a non-scarce resource in the same trade-off – between oranges and cashews. In this context, tunneling is said to occur if respondents have higher rates of substitution in exchanges involving the scarce resource (i.e. offering less money or water than oranges in exchange for the same quantity of cashews). The reason is that relative valuation should respond to the expected changes in the environment triggered by the sources of variation we draw upon. Second, in the word search games, subjects must correctly identify whether or not they heard specific words in a sequence of words narrated with audio distortion. Scores compare subjects' performances in words linked to the scarce resource(s) (e.g. *money* or *water*) to those involving neutral words (e.g. *husband* or *brother*). The higher the differential performance within subject, the higher our tunneling score. Last, for sensitivity to framing, we use a widely-deployed question that examines subjects' willingness to spend time to obtain the same discount (in monetary amount) on the baseline purchase price of the same item – except that the baseline price is high in one version, and low in the other, such that the discount varies in % terms (although constant in levels). Sensitivity to framing occurs if, despite the discount being identical in both version, subjects' willingness to spend time to obtain it is *not consistent* across the high-versus-low baseline purchase price/quantity scenarios.[30] We present respondents with question pairs involving such time versus money discount tradeoffs, for both non-scarce and scarce-resource items (water). Our tunneling score in this context is when respondents give *more consistent* answers across the high versus low price scenarios, when the item being purchased is a scarce one – i.e. they are *less* susceptible to framing when it comes to scarce resources.[31]

*Additional Outcomes***:**

We also analyze the effects of each source of variation on subjects' reaction times – accurately captured through the IVR technology – within each outcome category, as financial worries have

[30] Presumably, this happens because subjects think in relative terms, ignoring that the absolute trade-off between time and resources is identical across versions.

[31] The analysis of this variable is restricted to subjects who (i) answered both questions that offered these trade-offs, which were spread across different calls within each (monthly) wave of the survey; and (ii) were equally primed (or not primed) in both calls. For this reason, we have fewer observations for this variable.

been shown to deteriorate both accuracy and reaction speed in previous studies (e.g. Mani et al., 2013). Finally, we also examine the effects of each shock on money earned in the cognitive load and tunneling tasks, as a way to assess their net impact across these two dimensions. We present these additional results in the Supplementary Appendix II.

3.4 Estimation and Summary Measures

For each outcome, we estimate the empirical counterparts of β_j in equations (1), (2), and (3), where each outcome Y^j is indexed by municipality m, individual i and survey t:

$$\begin{cases} Y^j_{mit} = \alpha + \theta_m + \beta^1_j Priming_{mit} + u_{mit} & (1) \\ Y^j_{mit} = \alpha + \theta_m + \beta^2_j Rainfall_{mt} + u_{mit} & (2) \\ Y^j_{mit} = \alpha + \theta_m + \beta^3_j Payday_{mit} + u_{mit} & (3) \end{cases}$$

In equations (1) to (3), θ_m stands for municipality fixed effects; $Priming_{mit}$ equals 1 if individual i in municipality m was primed to worry about a drought at survey t, and 0 otherwise; $Rainfall_{mt}$ is a measure of negative rainfall shocks in municipality m at survey t; $Payday_{mit}$ is a measure of distance to payday for subject i in municipality m at survey t; and u_{mit} is an error term. We cluster standard errors at the individual level, in order to account for potential serial correlation in residuals.[32] Following Belloni et al. (2012), we use conventional standard errors for the effects of the post-LASSO *no-rainfall summary measure*.

All regressions include municipality-level fixed effects, but no wave or survey fixed effects nor individual fixed effects. In principle, we could include wave fixed effects in all specifications to increase precision, but in practice, these dampen the rainfall variation over the course of the rainy season considerably – so much so that LASSO does not pick *any* predictor of worries about rainfall in the presence of municipality *and* wave fixed effects. In Supplementary Appendix IV, we show

[32] Since priming is randomly assigned at the individual-call level, and since distance to payday is as good as random at the individual-call level, in principle there would be no need to cluster standard errors for inference on the effects of those shocks. In contrast, rainfall shocks as are good as random at the municipality-call level. Inference about the effects of those shocks is robust to clustering standard errors at that level.

that including individual fixed effects does not affect our point estimates – and even increase the precision of estimated coefficients.

Since we conduct a multiplicity of tests within each category, estimating separate regressions for each outcome would substantially inflate the probability of false positives above stated significance levels. Therefore, we build summary measures for each set of outcomes, following Kling, Liebman and Katz (2007). To construct these summary measures, we first normalize all outcomes to z-scores using the control group mean and standard deviation of each variable. Second, following Kling and Liebman (2004), we run seemingly unrelated regressions (SUR) to compute an effect size $\hat{\beta}$ for each summary measure, given by equation (4):

$$\hat{\beta} = \frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}} \tag{4}$$

In equation (4), $\hat{\beta}_j$ are the point estimates obtained for ordinary least squares (OLS) regressions of Y^j on a particular treatment variable, $\hat{\sigma}_{j_c}$ is the variance of that outcome for the control group, and K is the number of outcomes in that category. In other words, each estimate within a summary measure is weighted by the inverse of its variance.

4 Data Description and Balance Checks

In this section, we first examine the basic features of our data and then discuss issues related to balance across treatment and control groups and non-response in subsection 4.1. Subsection 4.2 presents the effects of each shock on worries about rainfall, an intermediate outcome underlying the mechanism of interest. This helps verify whether the three sources of variation we exploit – priming, rainfall shocks, and distance to payday – work in a manner consistent with predicted cognitive effects examined in the next section.

4.1 Balance and Non-response

We start with descriptive statistics of our sample, analyzing whether participants' characteristics collected at baseline are balanced across treatment conditions for priming and rainfall shocks.

Table D1 showcases that about 2/3 of subjects enrolled in our study are female, averaging 35 years old. Most participants indeed rely exclusively on rainfall for agriculture – less than 14% of them have access to irrigation. Rain-fed irrigation, combined with the irregular rainfall pattern in the region, sustains motivated beliefs about what determines a good rainy season: about 2/3 of farmers believe that the rainy season will be good if it rains on March 19th, the day of Ceará's patron saint, even though this rule of thumb is wrong about 70% of the time.[33] Slightly under 1/3 of participants own their plot, and only about 20% of them harvest cassava – a higher-value cash crop, which proxies for market-oriented farming. Almost 80% of our subjects report to be enrolled in Bolsa Família, and a similar share report to have signed up for Government index insurance (which pays out if harvest losses in the municipality are 50% or higher, see Lichand and Mani, 2019).

Even though priming is randomly assigned prior to each call, potential imbalances could arise from participants selectively hanging up after being primed about droughts at the beginning of a call. Table D1 shows that not to be the case: for baseline covariates, most differences are not statistically significant. For the only two that are (about the expected rate given that we test differences across 12 covariates), differences in the number of rooms and in participant's schooling across treatment and control are tiny (about 1.5% of the average of the control group in both cases), even though precisely estimated.

Table D2 displays balance tests for no rainfall 3 and 7 days prior to each survey. Once again, whenever there are statistically significant differences in baseline covariates across treatment and control, they are very small in magnitude. In any case, our estimates are very robust to controlling for all baseline controls.

Last when it comes to balance, Table D3 displays balance tests for payment within 3 and 7 days from each survey. In this case, we can investigate not only differences in covariates collected through our phone survey for observations matched with CadÚnico, but also in covariates based on administrative data for all observations in CadÚnico by 2015 in the municipalities we work

[33] Anthropologists have pointed out, in the context of such beliefs in Ceará, that "[t]he presence of rain prophets and the many natural 'signs of rain' to which rural people attribute great significance are testimonies to the *psychological anxiety* that the threat of drought engenders", Finan (2001, p. 6, emphasis added).

with. Once again, whenever there are statistically significant differences in baseline covariates across treatment and control, they are very small in magnitude.

Next, we analyze if any of the shocks we rely on leads to selective non-response. Table D4 presents the results of ordinary least squares (OLS) regressions with an indicator variable equal to 1 if survey call t was completed by subject i and 0 otherwise as dependent variable, and with each of our shocks as independent variables, in separate regressions.

While priming or distance to payday do not affect response rates, the absence of rainfall significantly affects non-response: a one standard-deviation increase in the no-rainfall summary measure leads to a 1.3 percentage-point *higher* probability of taking the call (from a baseline of 43.9 p.p.) in the full sample. The effect is significant at the 10% level, similar to the effect of no rainfall 3 days before the survey. In the Bolsa Família sub-sample, this response rate is 2.3 percentage points higher (panel B, Table D4). It seems that, when it rains, farmers are more likely to be on the field, and less likely to take our phone calls.

Selective non-response raises potential concerns with differences across treatment and control being driven by non-observable characteristics, e.g. if the marginal farmers who take the survey after it has rained recently are not as concerned with the harvest, and hence perform better for reasons unrelated with recent shocks. To deal with that concern, with rely on Lee (2009)'s method to bound treatment effects in the presence of selective non-response.

Last, Table D5 analyzes the marginal effects of baseline characteristics on the probability of completing each survey. Some participants' characteristics significantly affect the average probability of completing the surveys. For instance, being poorer or more highly educated both increase response rates, while having access to irrigation significantly decreases participation in our surveys. This could matter in the presence of heterogeneous treatment effects, which we analyze in subsection 4.5. In Supplementary Appendix VI, we show that re-weighting observations by the inverse of their probability of response yield results consistent with those of subsection 4.5.

4.2 Worries About Rainfall

In examining the psychological effects of income uncertainty and low levels of income, our implicit conjecture is that it is worries from coping with these challenges that affect attention allocation and decision-making. To thread this causal chain, we begin by ascertaining how worries themselves are affected by exposure to priming and rainfall shocks. As an outcome measure for this, we use survey questions about the extent to which someone in the household worried about future rainfall in the previous week, or the extent to which the respondent is worried about being able to cope with upcoming household bills (see Appendix A).[34] We normalize these variables to z-scores in analyzing how each measure of worries responds to priming and rainfall shocks.

In Table 1, all columns use worries about rainfall as the dependent variable, except for column (5), which uses worries about household bills (see Appendix A for the full script of the survey instruments). Columns (1) to (4) consider the full sample, estimating the effects of priming on worries about rainfall. Columns (5) and (6) restrict attention to March and April (the "early waves"), when uncertainty about the rainy season still is unfolding. Columns (7) and (8) estimate the effects of rainfall shocks on worries about rainfall, for the full sample and the Bolsa Família sample, respectively. All columns are OLS regressions, with standard errors clustered at the individual level.

We find that priming increases worries about rainfall by 0.05 standard deviations (column 1). This effect is noisily estimated, but becomes larger and statistically significant at the 10% level when we include wave fixed effects (column 2). In terms of relative magnitudes, this is about 1.5 times the impact of losing access to irrigation on (rainfall) worries, and equivalent to the impact of losing 20% of one's harvest or having about 1 day less of rainfall in the previous week (magnitudes based on a cross-sectional comparison within our sample). We also note that the effects of priming on worries are concentrated early in the rainy season (column 3): it peaks in the first wave, and then decreases until it basically disappears between May and June, when

[34] Even though the question on worries about rainfall is phrased in the past (to reflect potential responses to past shocks), the answer can naturally be influenced by ongoing worries by the respondent, potentially affected by other factors such as priming.

uncertainty about the amount of rainfall has been resolved.[35] The responsiveness to priming increases with harvest losses in the previous season (column 4). Columns (5) and (6) highlight not only that the experimental manipulation works, but also that its effect is sharply confined to the domain of interest: early on, priming affects worries only about future rainfall, but *not* about coping with household bills (because it is rainfall that will determine harvest outcomes later).

The effect of the no-rainfall summary measure is much larger in magnitude (4-fold that of priming), and very precisely estimated (column 7; statistically significant at the 1% level). This could be due to the fact that it is a real-world shock, and also because LASSO was set up precisely to select the rainfall shock variables most predictive of worries about rainfall. The coefficient of priming changes little in the presence of rainfall shocks, consistent with the fact that both shocks are independently distributed (priming was randomized by design). Their interaction is not statistically significant. As we show in Table 3, this does not mean that the shocks cannot compound to magnify their effects on cognitive function. Rather, our findings here are consistent with a *threshold* model of worries, in which (real-world) *stimuli above a certain cutoff* make scarce resources top-of-mind; once this happens, additional (priming) stimuli may not create additional worries, although they could still affect cognitive function directly. This could also explain what we observe for the Bolsa Família sample (column 8): priming and distance to payday do not have a marginal impact on worries – presumably because, in this poorer sub-sample, worries are already above that threshold, and particularly so in the absence of rainfall.[36]

Having established that the experimental and naturally occurring shocks do influence worries among respondents, we now proceed to examine the effects of these factors on their cognitive function. We would like to clarify that we do not estimate an instrumental variable model (with rainfall as the instrument for worries) given that rainfall could independently affect cognitive function through other channels.

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[35] The fact that worries increase on average with every additional wave could be explained by the fact that rainfall in Ceará in 2015 was below-normal for the fifth consecutive year, with harvest losses as widely prevalent and as large as those in the previous 4 years. Interestingly, the average effect of priming is increasing in municipalities' harvest losses in the *previous* year – and basically zero where no losses took place – consistent with an *affect* mechanism whereby priming activates memories of previous negative experiences.

[36] An alternative reason is *ceiling* effects. In any given wave, over 85% of our subjects report being at least somewhat worried, limiting the extent to which shocks could increase measured worries even further. This figure is very similar within the Bolsa Família sub-sample.

5 Effects of High Income Uncertainty on Cognitive Function

We describe the effects of income uncertainty for the two sets of outcome measures described in detail in Section 3, first for cognitive load (subsection 5.1) and then for tunneling (subsection 5.2). Robustness checks are summarized in subsection 5.3. Last, subsection 5.4 presents the results for the effects of income uncertainty within the Bolsa Família sub-sample – so that we can then compare these against the impact of lower income levels under poverty, which can only be computed for this sub-sample (presented in Section 6).

5.1 Cognitive Load Under High Income Uncertainty

In Table 2, cognitive load outcomes are normalized such that negative coefficients indicate worse performance in individual tests. Column (1) presents the cognitive load effect of priming, column (2), that of the no-rainfall summary measure, and column (3), that of the two shocks together and their interaction. Columns (4) and (5) present the effects of two intuitive measures of rainfall shocks: no rainfall 3 days prior and 7 days prior to the survey, respectively, which allow us to verify whether effect sizes decay with distance from the time of the survey, as one would expect. Column (6) presents the effects of municipal-level harvest losses restricting the sample to the last wave – when such output losses have been realized. Results displayed in all the columns are based on SUR regressions, with standard errors clustered at the individual level and controls included for all baseline characteristics.[37]

As the table shows, priming generates cognitive load, decreasing performance by 0.046 standard deviation (column 1; statistically significant at the 5% level). The loss in cognitive performance is sizable: it is equivalent to the performance gap between those with a high-school education and those with an elementary school education (in a cross-sectional comparison). The cognitive load of rainfall shocks is about twice as large as that of priming, and very precisely estimated (column 2; statistically significant at the 1% level), suggesting a direct link between worries and cognitive function (since LASSO picks the rainfall shocks most predictive of worries

[37] To compute SUR coefficients in each column, following equation (4), we first estimate coefficients for the effects of each shock on each of the different components of the cognitive load summary measure. For each component, the number of observations sums the number of valid responses for that task *across all waves*. In the table, we display the *minimum* number of observations across components of the cognitive load summary measure.

about rainfall). For cognitive load, the effects of these two shocks individually is not magnified by their joint occurrence (column 3). This is consistent with a threshold model of worries, discussed in subsection 4.2, whereby once a shock drives a certain dimension to the top of the mind, additional shocks will no longer generate worries and cognitive load (even though they might still affect bandwidth reallocation; see subsection 5.2). Next, columns (4) and (5) document that more recent occurrences of no rainfall (3 days, rather than 7 days prior to the call) have a larger adverse cognitive impact. Last, column (6) allows us to benchmark the cognitive effects of priming and rainfall shocks to those of harvest losses. Shocks to agricultural output that could not be anticipated by the onset of the rainy season have a large and significant negative impact on cognitive performance. The effect of priming is large also by that account: its coefficient in column (1) is *nearly 25%* of the effect size of harvest losses at the end of the rainy season.

5.2 Tunneling Under High Income Uncertainty

The results for tunneling outcomes in Table 3 are normalized such that negative coefficients indicate lower relative attention to tasks involving scarce resources, lower valuation of scarce resources, and higher sensitivity to framing biases in tasks involving scarce resources. Column (1) presents the effect of priming, column (2), that of the no-rainfall summary measure, and column (3), both the independent and interaction effects of these two shocks. As in Table 2, columns (4) and (5) present the effects of two intuitive rainfall measures, no rainfall 3 days prior and 7 days prior to the survey, respectively. Column (6) presents the effects of municipal-level harvest losses, restricting the sample to the last wave (after the end of May, when output losses are accounted for). Results displayed in all the columns are based on SUR regressions, with standard errors clustered at the individual level and controls included for all baseline characteristics.[38]

We find that priming generates tunneling, improving cognitive performance in tasks involving scarce resources by 0.04 standard deviation (column 1; statistically significant at the 5% level). The effect of rainfall shocks within this dimension is very similar to that of priming, with

[38] In the table, we display the *minimum* number of observations across components of the cognitive load summary measure; see footnote 37.

an effect size of 0.043 (column 2; significant at the 10% level), once again in line with a direct link between worries and cognitive function. For tunneling, the effects of the shocks in isolation are magnified by their joint occurrence (column 3) to a considerable extent: a recent experience of a negative rainfall shock magnifies the impact of priming *nearly two-fold*. Comparing across columns (4) and (5) in Table 3 confirms that a more recent adverse rainfall shock results in greater reallocation of mental bandwidth to tasks involving scarce resources: effects sizes decay with distance to the call (and it is only for shocks 3 days prior to the survey that tunneling effects are significant, with an effect size similar to that of priming). Last, column (6) shows that, by the time harvest losses become known, they no longer significantly induce attention reallocation (even though they do increase cognitive load considerably, as shown in section 5.1).

5.3 Robustness Checks

Table 4 shows that the results described above in Tables 2 and 3 are robust to concerns arising from selective non-response to rainfall shocks, applying Lee (2009)'s procedure to bound treatment effects.[39,40]

Figures 1 and 2 present disaggregated results on the impacts of each shock on the score of each task designed to capture cognitive load and tunneling, respectively. For both sets of outcomes, it is clear that the effects of either shock are not driven by any specific component of the summary measure alone.

5.4 Cognitive Effects of High Income Uncertainty Within the Bolsa Família Sub-sample

Next, we examine whether the results described earlier in this section also hold for the sub-sample of respondent households for whom we were able to find a match with their Bolsa Família payday details. Looking at this sub-sample sets the stage to directly compare the effects of high income uncertainty (as captured by priming and rainfall shocks) with those from low income levels (as

[39] A limitation of the Lee bounds' procedure is that it can only be applied to binary treatment variables; hence, we cannot apply it to the no-rainfall summary measure. We therefore apply it to two alternative rainfall measures: no rainfall 3 days prior and 7 days prior to each phone surveys.

[40] Supplementary Appendices IV and V present additional robustness checks, including individual fixed effects and using an alternative measure of rainfall shocks. It also presents the distributional effects of priming, rainfall shocks and payday variation, and additional results on reaction times and money earned in the experiments.

captured by distance to Bolsa Família payday), presented in Section 6. As we did for the full sample, we first verify whether the payday shock affects worries among our matched Bolsa Família respondents and then present results on the cognitive load and tunneling outcomes (subsection 6.1).

In Table 5, each cell stands for a different regression. Panel A presents results on worries about rainfall and household bills and Panel B, the results on cognitive load and tunneling. In this subsection, we concentrate on the first two columns. Across both panels, column (1) presents the effects of priming, and column (2), those of the no-rainfall summary measure. As before, outcomes are normalized such that negative coefficients indicate worse performance in attention, memory and impulse control tests; and lower relative attention in tasks involving scarce resources, lower valuation of scarce resources, and higher sensitivity to framing biases in tasks involving scarce resources. Results displayed in all the cells are based on SUR regressions, with standard errors clustered at the individual level and controls included for all baseline characteristics.[41]

A comparison of the coefficients in Panel A shows that the effects of income uncertainty on worries about rainfall and bills within the Bolsa Família sub-sample are qualitatively similar to those for the full sample (Table 1, columns 1, 6 and 7): the main worry-inducing effect – both about rainfall and managing household bills – comes from the rainfall shock. At the same time, we remind the reader that, under a *threshold* model of worries, further sources of worry (i.e. as through priming or distance to payday) may have no marginal impact on worries but could still affect cognitive function.

Coming to cognitive load and tunneling (Panel B), the direction of impact is the same as that for the full sample. However, the magnitude of the cognitive load impact of the priming and rainfall shocks is roughly 50% larger in this sub-sample (respectively 0.064 versus 0.0458 in column 1 of Table 2, and 0.15 versus 0.108 in column 2 of Table 2). Tunneling effect sizes are similar to those estimated in the full sample, albeit measured more noisily.

[41] In the table, we display the *minimum* number of observations across components of the cognitive load summary measure; see footnote 37.

These results for the Bolsa Família sub-sample set the stage for a comparison of the psychological impact of high income uncertainty with those due to low levels of income. We examine the latter issue in the next section.

6 Effects of Low Income Levels on Cognitive Function

We now turn to the effects of income levels on cognitive function, taking advantage of respondents' income fluctuations around their Bolsa Família payday – which is randomly assigned. We estimate the psychological impact of income level shocks by comparing those (randomly) surveyed *before* their payday relative to those surveyed *after*, where the latter group experiences a temporarily higher income level in face of difficulties with consumption smoothing under severe liquidity constraints (in our sample, roughly 80% the individuals answer they are interested in listening to credit offers in any given call).[42] We describe the effects of low income levels on worries, cognitive load and tunneling in subsection 6.1.

6.1 Worries, Cognitive Load and Tunneling

Table 5 (columns 3 to 5) presents the effects of payday shocks on the outcomes of interest: worries (Panel A) and cognitive load and tunneling (Panel B). The three payday variables compare before versus after payday differences in worries and cognitive function. Column (3) presents the effects of a linear measure of distance to payday, while columns (4) and (5) present the effects of two intuitive measures of payday shocks: payment within 3 and within 7 days from the survey, respectively. In column (4), we restrict the sample to observations distant *at most 3 days* from their their Bolsa Família payment (before or after) and, in column (5), to observations distant *at most 7 days* from their payment. We introduce the latter to allow for non-linear effects of distance to payday on cognitive function; we pick the 3- and 7-day windows just as natural counterparts to the indicator variables of rainfall shocks.[43]

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[42] As in subsection 5.4 above, our analyses are based on the subset of respondents whom we were able to match to their Bolsa Família registry details (as described in subsection 3.2.2).

[43] We define the distance to payday indicator variables as within 3 and 7 days rather than exactly on 3 and 7 days before payday (as the no rainfall variables defined) due to sample size issues; see Table C4.

In Panel A (columns 3-5), we see that none of the three payday shock variables has a significant effect on worries about household bills. In Panel B, we see that, on average, a lower income level (before payday) does not systematically generate cognitive load either (first row of Panel B, columns 3-5). This is not merely an artifice of smaller sample sizes: in contrast to the payday shock, the effects of priming and rainfall shocks on cognitive load within this Bolsa Família sub-sample (Panel B, columns 1 and 2) are actually *more* adverse than in the full sample (statistically significant at the 10% and 1% levels, respectively). In fact, the coefficients of the indicator variables of distance to payday on cognitive load are not just insignificant; they are actually *positive*.

At the same time, these same indicator variables do have large and statistically significant effects on tunneling (second row of Panel B, columns 3-5), showing modest decay with distance to payday: within 7 days of the Bolsa Família payment, relative performance in tasks involving scarce resources improves by 0.210 standard deviations, compared to 0.224 standard deviations within 3 days of the payment (both coefficients significant at the 5% level).

Figure 3 reports both individual components and summary measures of the cognitive load and tunneling outcomes for the Bolsa Família sample in a 7-day-window before relative to after payday. Figure 4 reports non-parametric estimates for the summary measures of cognitive load and tunneling before relative to after payday, in symmetric time windows ranging from 1 to 15 days around payday.[44] Both figures confirm that the pattern of payday shock effects described in Table 5 is not an artifact of the influence of particular components of the summary measure or of specific distances from a respondent's Bolsa Família payday respectively.[45] The bottom line is that, on average, having a lower level of income before payday (relative to after) has no adverse impact on cognitive load, but induces large and statistically significant attention reallocation, more so the closer to payday (as shown in Figure 4).

[44] For each 'dot' coefficient in Figure 4, we hold distance to payday fixed, restricting attention to participants *at most D days* before versus after payday, with D ranging from 1 to 15. Each dot represents the value of the coefficient, while bars reflect the 95% confidence intervals.

[45] The performance patterns we observe are robust to excluding individual components from the cognitive load or tunneling summary measures; see Supplementary Appendix II.

7 Do Low Income Levels Have No Adverse Psychological Effects On the Poor?

One possible reason why income level shocks from Bolsa Família payouts have no cognitive load effects on average could be heterogeneity: within our sample, respondents may vary in their ability to access credit or in other sources of income. While we do not observe income or access to credit directly at the individual level, in section 7.1 we examine heterogeneous treatment effects by municipality-level per capita income (from the 2010 Census) for all three shocks, taking advantage of variation across the 47 locations of our study. Importantly, subsection 7.2 then discusses how our findings allow us to better understand and reconcile apparently inconsistent previous evidence from the literature on poverty and cognitive function.

7.1 Heterogeneous Effects By Municipalities' Per Capita Income

Table 6 reports heterogeneous cognitive effects of each shock ('treatment') examined in Table 5 by municipality-level per capita income, introducing an additional interaction term between the two (with municipalities' per capita income from the 2010 Census, expressed in natural logarithms). Panel A presents the results for cognitive load, and Panel B, those for tunneling. Outcomes are normalized such that negative coefficients indicate worse performance in attention, memory and impulse control tests; and lower relative attention in tasks involving scarce resources, lower valuation of scarce resources, and higher sensitivity to framing biases in tasks involving scarce resources. As before, each cell is a SUR regression, with standard errors clustered at the individual level and controls included for all baseline characteristics.[46]

We find that, for one of our measures of low income level shocks (payment within the next 7 days), effects of cognitive function vary considerably with income. Farmers in the poorest municipalities experience high cognitive load 7 days before payday (row 1, column 7, significant at the 5% level). In fact, among the poorest municipalities, the effect size from low income level shocks is more than three times as large as those from income uncertainty (induced by priming or adverse rainfall shocks; row 1, columns 1 and 4). At the same time, cognitive load effects right before payday, when the budget is likely to be tighter, decrease sharply with per capita income

[46] In the table, we display the *minimum* number of observations across components of the cognitive load summary measure; see footnote 37.

(row 2, column 7; significant at the 5% level), eventually becoming *positive* at income levels where a larger fraction of our sample lies. Combining these negative effects on the poorest with the positive effects among the richer segments of our sample is what generates the overall average null effect on cognitive load in Table 5 (column 5 in panel B). Panel B (row 2) in Table 6 shows that, in contrast to cognitive load, the tunneling effects of payday shocks do *not* vary systematically with income (columns 6-7); neither is there income-based heterogeneity in the effects of priming and rainfall shocks.

Figure 5 plots the predicted effects of the three shocks – priming, no rainfall and distance to Bolsa Família payday – using coefficient estimates in Table 6, across the full income range in our sample (Panels A, B and C, respectively). For ease of interpretation, with the goal of highlighting different patterns, we pick the variables with the largest effect sizes within each set of shocks: rainfall 3 days prior, in Panel B, and payment within the next 7 days, in Panel C. In each panel, black lines depict cognitive load effects and grey lines depict tunneling effects, with negative values signifying worse outcomes.

A quick visual comparison across these panels shows several noteworthy points. First, income uncertainty from priming and rainfall shocks (Panels A and B, respectively) creates cognitive load *and* tunneling effects across most of the per-capita income range (identified by the vertical dotted line at zero on the Y-axis). This holds true for the tunneling effects of the payday shock as well (Panel C, grey line); however, the cognitive load from this income level shock is concentrated *only among the poorest municipalities* (to the left of the dotted vertical line). The coexistence of cognitive load *and* tunneling in most of the income range is uniquely consistent with the *scarcity* mechanism. In turn, the absence of cognitive load at moderate income levels soon before payday (in Panel C) is consistent with other mechanisms, such as the hopefulness and anticipation it creates (as in Caplin and Leahy, 2001).

Second, we draw attention to the effects at each end of the income range. It is fair to say that the poorest municipalities suffer the most adverse psychological effects of low and uncertain incomes. Cognitive load is highest in this income range across all three panels. It is the poorest alone who endure cognitive load from a low level of income; in fact, the effect size of being 7 days away from payday on cognitive load is *much greater* than those they endure from priming or no rainfall. The ability to tunnel on tasks involving scarce resources is also the weakest in the lowest income range. So much so that the relative performance of the poorest in tasks involving scarce resources actually *worsens* when faced with a negative rainfall shock (Panel B, to the left of the vertical dotted line).[47] In contrast, the richest segment of our sample faces no cognitive burden from priming, and even *benefits* from it (Panel A, to the right of the dotted vertical line).[48]

To summarize, the pattern that emerges from our findings indicates that, for most of the income range that characterizes the poor in our sample, it is high income uncertainty rather than low income levels that drives poverty's psychological tax. However, among the poorest, *both* dimensions of poverty have a significant cognitive impact – with a larger adverse impact coming from low levels of income itself (at least for one of our measures of payday shock).

7.2 Reconciling Results From Existing Literature

The above results offer valuable insights into possible reasons for differences in the findings of previous studies – specifically Mani et al. (2013) and Carvalho, Meier and Wang (2016) – and suggest that they can be reconciled after all. The first study surveyed Indian sugarcane farmers who faced low income levels and uncertainty in the timing and/or amount of harvest payments. The second study focused on US respondents who faced low income levels before payday(s), where the date(s) and amount(s) of payment were known at the time of the survey. In other words, Mani et al. (2013) documents the cognitive load effects of low *and* uncertain incomes while Carvalho, Meier and Wang (2016) documents these same effects due to low income levels alone, on a sample of US respondents who are not as poor as the poorest municipalities in our sample or the farmers in India.[49] Based on our results reported in Table 6, the differences in findings across these two studies could thus be driven by the absence of income uncertainty and/or the higher income levels among the US respondents, as compared to Indian sugarcane farmers studied in Mani et al (2013) or the Brazilian farmers in the present study.

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[47] This is a phenomenon referred to as *choking* (mistakes driven by high stakes, as in Ariely et al., 2009) – the *opposite* of tunneling.

[48] This pattern is consistent with *rational inattention* more broadly– since it enhances subjects' overall performance in face of attention reallocation.

[49] Neither paper studies tunnelling effects – which we find significant evidence of, due to *both* high income uncertainty and low levels of income.

8 Does Tunneling Help the Poor Overcome Cognitive Load On Balance?

So far, we have separately reported the cognitive load and tunneling effects of income uncertainty, both of which are found to be present in most of the sample. However, given that these effects go in opposite directions – cognitive load is a general deterioration of performance, while tunneling is a relative improvement –, a question that naturally arises is about their *overall* impact on cognitive performance: does tunneling on scarce-resource tasks *compensate* for cognitive load effects, such that the *global effects* of attention reallocation are actually positive within certain tasks – those involving scarce resources?

We can use the cognitive load and tunneling estimates across the per capita income distribution from Table 6 to compute global cognitive effects of each shock on *different types of decisions*. In the case of *regular tasks* (those not involving scarce resources), the overall cognitive impact is simply given by the cognitive load effect size of each shock. In the case of tasks or decisions *involving scarce resources*, we compute global effects by simply *adding up* cognitive load and tunneling effect sizes of each shock. Since cognitive load captures changes in *absolute* cognitive performance, while tunneling captures changes in *relative* performance, adding those two effects captures the absolute cognitive performance in tasks "inside the scarcity tunnel". We translate (added up) effect sizes measured in standard deviations (in Table 6) into percentile gains or losses to provide a more concrete interpretation of global cognitive effects in terms of changes in the ranking of cognitive scores.[50]

Figure 6 summarizes these computations by displaying percentile changes in overall cognitive performance in response to each shock by per capita income levels, across different types of tasks or decisions. The black line showcases the percentile change for regular tasks (i.e. those not involving scarce resources).[51] For tasks involving scarce resources, the grey line adds up the effects of cognitive load and tunneling by per capita income level, in each corresponding Panel of Figure 5, and then converts that sum to percentile changes. As an example, in Panel A of

[50] Doing so also allows us to take into account the fact that effect sizes do not translate linearly into changes in rankings. In order to compute percentile changes, we assume a normal distribution for cognitive scores, a hypothesis that is not rejected in the data.

[51] In each Panel A-C of Figure 6, it coincides with the percentile changes associated with the effect sizes of cognitive load for each per capita income level in each corresponding Panel of Figure 5.

Figure 5, the predicted cognitive load effect size of priming for farmers at the poorest municipality in our sample is -0.18 standard deviations; the corresponding effect size of tunneling is 0.02 standard deviations. In Panel A of Figure 6, these effect sizes translate into a 7 percentage-point decrease in performance due to priming in regular tasks and a 6.2 percentage-point decrease in performance due to priming in tasks involving scarce resources. These magnitudes simply reflect the percentile change in performance of moving from zero to -0.18 in a standard normal distribution in regular tasks, and of moving from zero to -0.16 (= -0.18 + 0.02) standard deviations in a standard normal distribution in tasks involving scarce resources.

Figure 6 shows that, for those at the bottom of the income distribution, the *net effect* of either shock to income uncertainty (priming, in Panel A, and rainfall shocks, in Panel B) on cognitive performance is negative *across all tasks* – including those that benefit from tunneling on scarce resources. In Panel A, the grey line crosses zero at USD 64.50 / month; over 40% of our sample lives in municipalities with per capita income below that threshold. In Panel B, the grey line crosses it only at USD 92 / month, below which over 90% of our sample lies. This implies that, for almost half of the farmers in our sample, tunneling effects *do not overturn* cognitive load from priming high income uncertainty *regardless* of the real-world distribution of the two types of tasks (i.e. those involving scarce resources and those not); when it comes to uncertainty triggered by rainfall shocks, the same is true for *nearly all respondents*. For those at the top of the income distribution, the overall cognitive impact of the shock (both priming and rainfall) depends upon the *real-life* frequency of decisions involving scarce versus non-scarce resources. The wealthier the municipality, and the higher the share of decisions involving (relatively) scarce resources, the lower the likelihood of an overall adverse cognitive impact.[52,53]

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[52] To address this question, we could have also used data on the performance of the top 25% of performers in our phone surveys, since they were incentivized with a monetary reward paid in the form of additional air-time credit – with money earned by these top performers as our outcome measure for the global impact of attention reallocation in face of each shock (those results are presented in the Supplementary Appendix). There would be two caveats from relying on this approach. First, we can only say something on the global effects of attention reallocation for the top-performers (given the structure of incentives), who are disproportionately concentrated in municipalities with higher per capita income. Second, the computation of the net effect of each shock on cognitive performance depends on the specific distribution of tasks involving scarce resources or not in our *phone surveys*, which might be very different from the relative distribution of those types of tasks in the *real world*.

[53] Other papers document that seemingly positive tunneling effects can also be globally inefficient for different reasons: Shah, Shafir and Mullainathan (2015) and Lichand et al. (2019) show that tunneling on short-term outcomes comes at

When it comes to payday variation (Panel C), net cognitive effects are negative across all tasks *only for the very poor*: while the black line crosses zero at USD 62 / month (below which about 40% of our sample lies), the grey line crosses it at USD 44.50 / month (below which only about 5% of our sample lies). For farmers in the bottom 5% of the per capita income distribution, the overall cognitive effects of low income levels are *negative* regardless of the real-life distribution of tasks.

Taken together, the bottom line is that, among the very poorest 5% of farmers in our sample, high income uncertainty and low income levels *both* worsen cognitive performance overall regardless of the real-life distribution of tasks involving scarce resources or not. Low and especially uncertain incomes under poverty make these individuals both 'penny wise and pound foolish'. For all other farmers, for whom the main impact of poverty comes from income uncertainty, the overall effect depends on two factors: their per capita income and the relative frequency of tasks involving scarce resources. Lower levels of both factors increase the likelihood of adverse overall cognitive effects.

9 Discussion and Conclusions

In this paper, we study the impacts of two key aspects of poverty – high income uncertainty and low levels of income – on cognitive function, using a combination of survey experiments and naturally occurring shocks. With regard to income uncertainty, the mechanism we study here – attention allocation – is distinct from conventional rational responses involving risk aversion. It predicts adverse effects not just on current decisions specific to the domain of risk, but across *all decisions*, including those mapping into future states *completely unrelated* to such uncertainty.

We find significant adverse effects of income uncertainty among the poor, as captured both through survey (priming) experiments and real-life negative rainfall shocks. These are distinct from the adverse effects of low income levels (captured through distance to payday), which are concentrated among the poorest segment of our study sample. The simultaneous increase in farmers' cognitive load together with better performance in tasks involving scarce resources

the expense of large long-term losses with a net reduction in earnings. Kaur et al. (2019) document an increase in workers' mistakes before payday.

(tunneling) supports the interpretation that these effects are driven by reallocation of mental bandwidth, consistent with the *scarcity* mechanism. This paper is the first to provide evidence that the predictions from this mechanism carry over from actually having too little to the *risk* of having too little as well. Importantly, we find that income uncertainty is the *key driver* of poverty's psychological tax.

Our findings on cognitive load are in line with many previous studies about the effects of scarcity on psychological outcomes (Mani et al., 2013; Shah, Shafir and Mullainathan, 2015; Haushofer and Fehr, 2014). Our design improves upon the existing literature in three ways. First, we combine exogenous natural variation with randomized survey experiments *simultaneously* in *the same setting*. Second, our psychological tests are undertaken within 5 minutes from priming, ruling out alternative mechanisms that could confound the effects of worries – for instance, differential nutrition or sleep. Third, we are able to assess both cognitive load *and* tunneling allowing us to distinguish across alternative mechanisms.

An important contribution of our findings is to help reconcile seemingly contradictory evidence across previous studies – notably, Mani et al. (2013) and Carvalho, Meier and Wang (2016). We show that these can be reconciled by differences in exposure to income uncertainty as well as in income levels across the two study samples.

Are these psychological effects of poverty first-order? There are two reasons why this is likely to be the case. First, the impact of worries on cognitive function that we find in this setting are sizable. The gap in cognitive performance across farmers differentially affected by rainfall risk is equivalent to that between farmers in municipalities with no harvest losses and those in municipalities with about 25% losses at the end of the rainy season. Second, in any given year, only some farmers are actually hit by a drought (in Ceará, for instance, 1/3 of municipalities are affected each year on average), whereas *all of them* are *always at risk*. Given that cognitive function lies at the foundation of every decision, these large effect sizes could imply significant efficiency losses across several decision domains, irrespective of whether such risk is realized.

Could those psychological effects generate poverty traps? Ongoing work sheds light on these issues, by analyzing the psychological consequences of poverty for productivity (Kaur et al., 2019) and on investments in children's human capital (Lichand et al., 2019). This is a promising avenue for future research, alongside interventions that could help mitigate those psychological effects by adapting the environment in which the poor make those decisions, from providing insurance (Lichand and Mani, 2019) to making the relevant decision features top-of-mind (Lichand et al., 2019).

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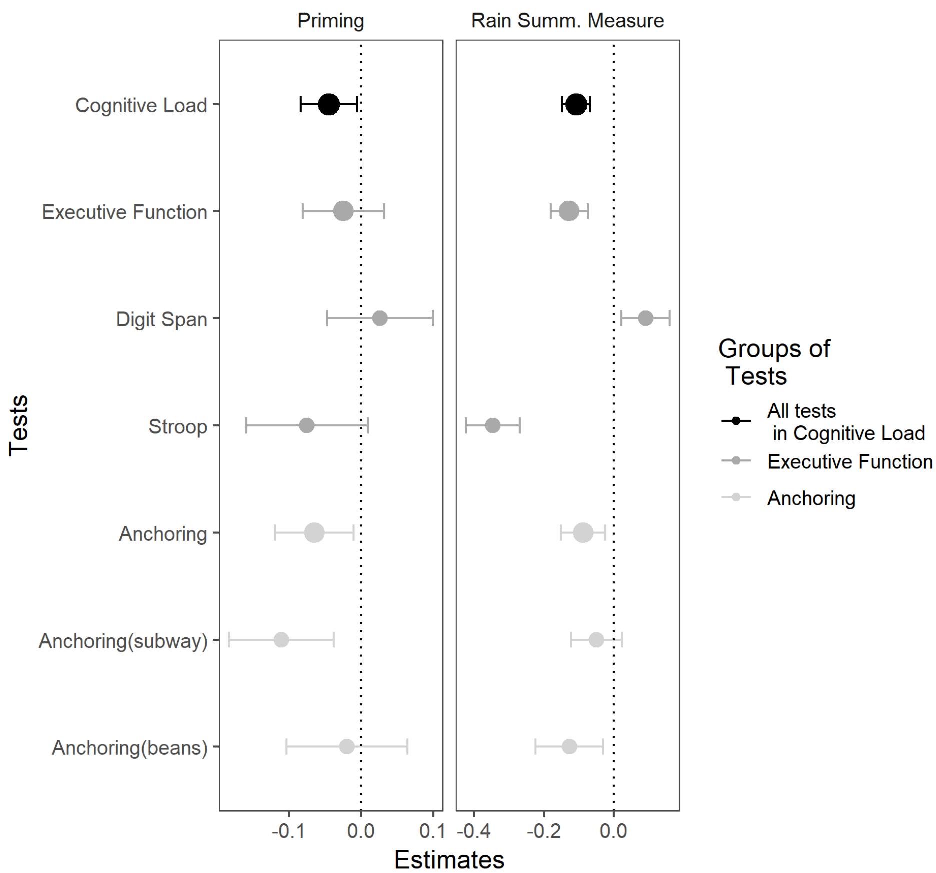
Figures

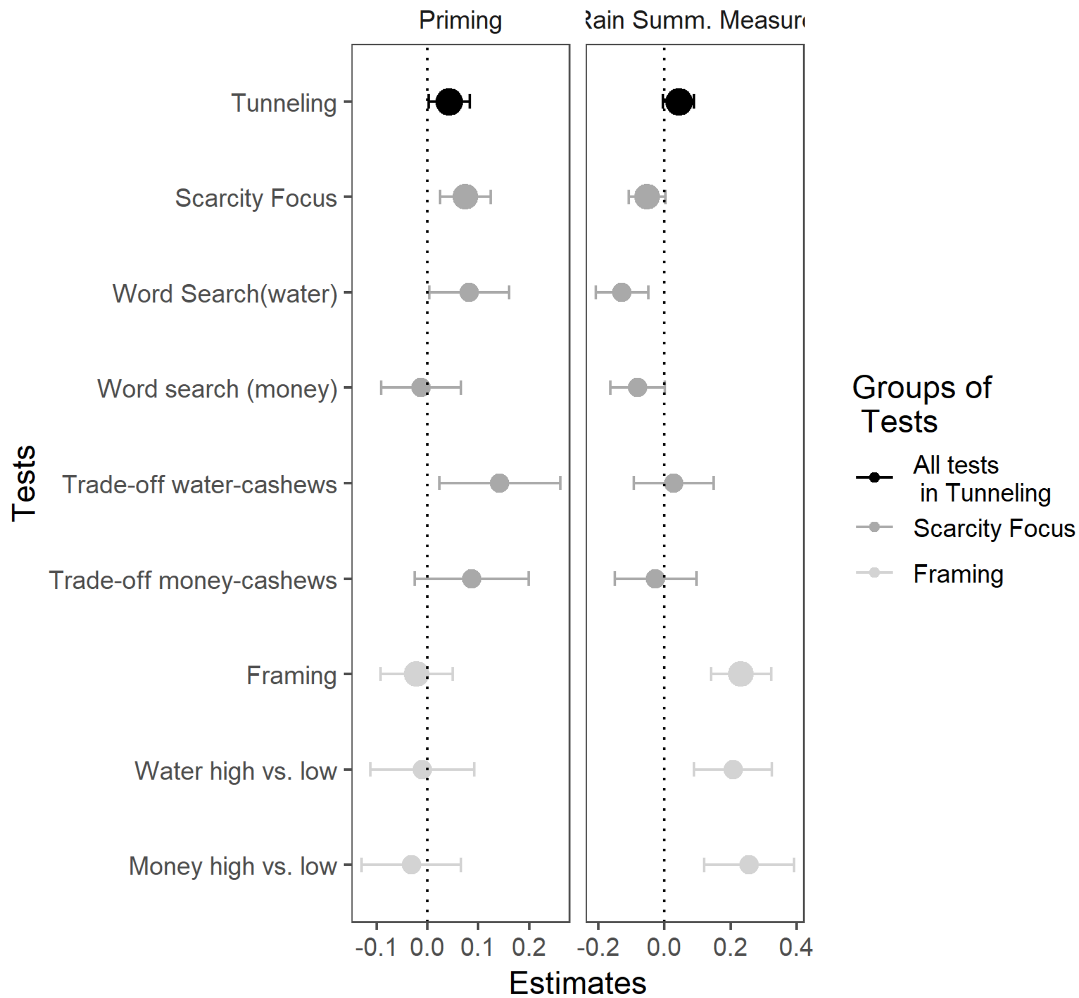
Figure 1 – Effects of priming and rainfall shock on the components of cognitive load



Notes on Figure 1:

1. The figure displays coefficients and 95% confidence intervals for summary measures and for all components under each category;
2. All estimates are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for executive functions and anchoring, including municipality fixed effects; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure component. See Appendix A for the definition of each variable;
3. Outcomes are normalized such that negative values mean worse cognitive performance (lower attention, memory and impulse control, and higher sensitivity to anchoring).

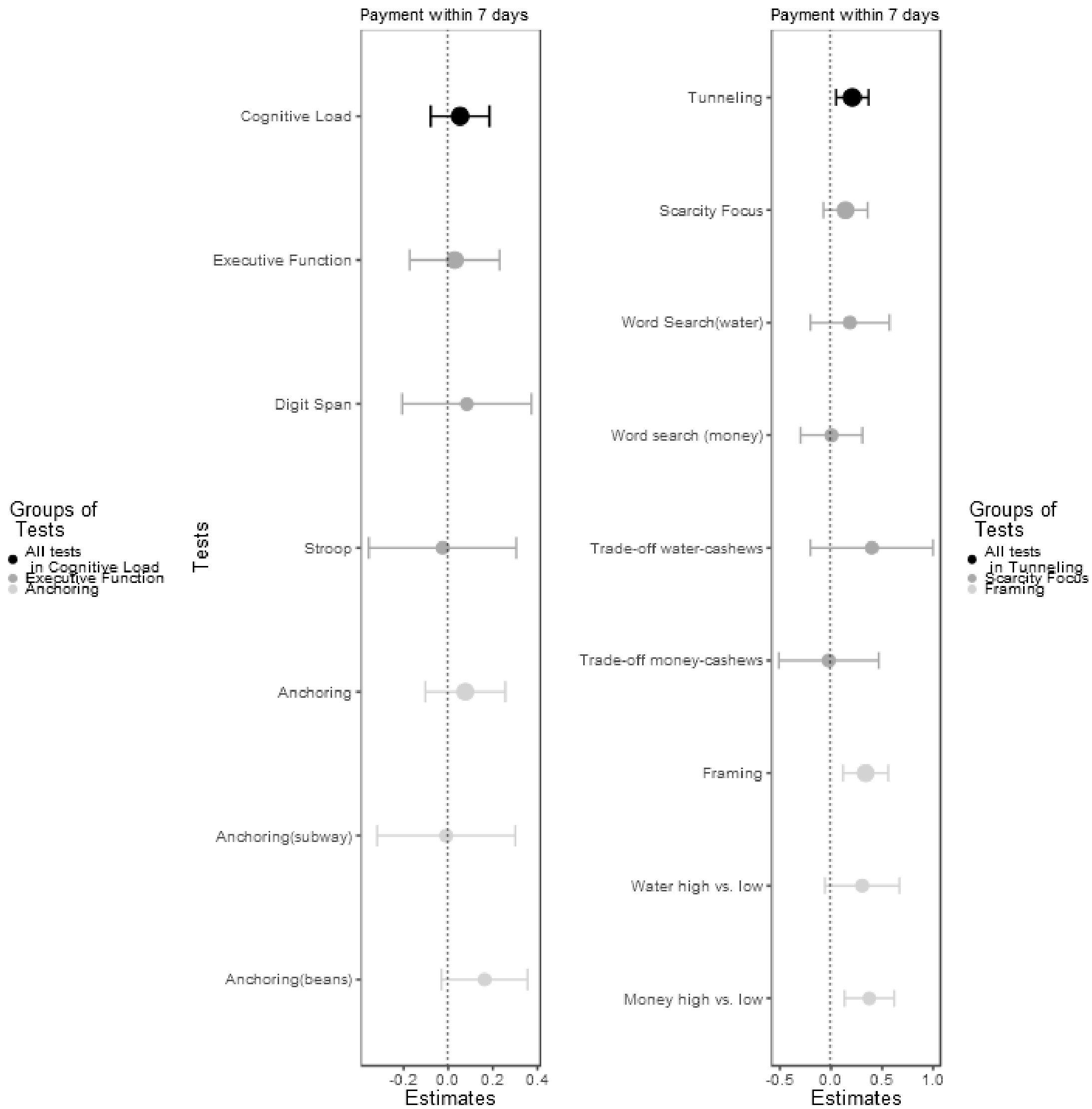
Figure 2 – Effects of priming and rainfall shock on the components of tunneling



<u>Notes on Figure 2:</u>

1. The figure displays coefficients and 95% confidence intervals for summary measures and for all components under each category;
2. All estimates effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for focus and framing, including municipality fixed effects; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure component. See Appendix A for the definition of each variable;
3. Outcomes are normalized such that positive values mean better relative cognitive performance (higher relative attention towards scarce resources, higher valuation of scarce resources, and lower sensitivity to framing biases in tasks involving scarce resources);

Figure 3 – Effects of payday shock on components of cognitive load and tunneling (Bolsa Família sub-sample)

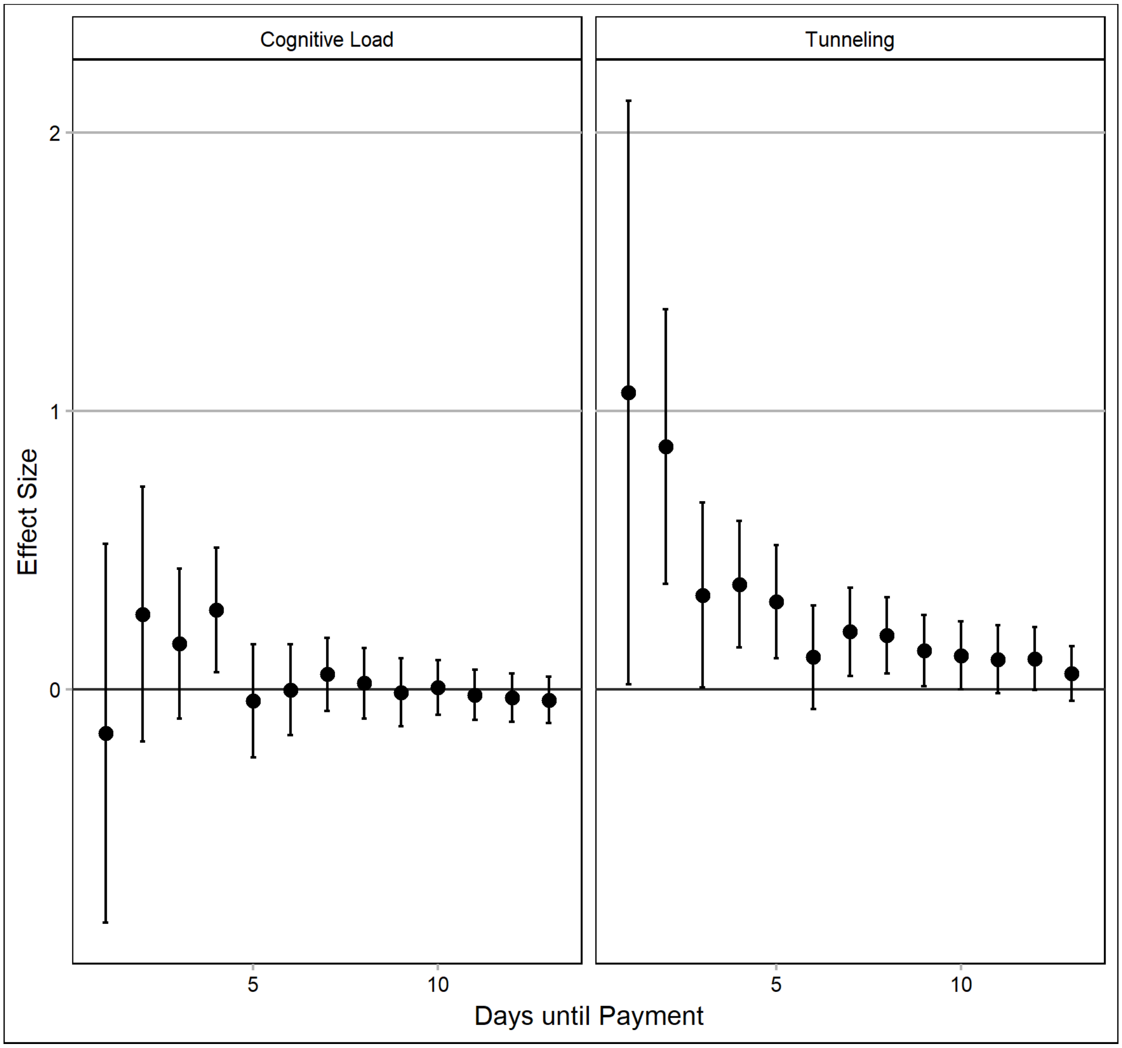


<u>Notes on Figure 3:</u>

1. The figure displays coefficients and 95% confidence intervals for summary measures and for all components under each category;
2. All estimates are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for executive functions and anchoring, in the left-hand side, and for focus and framing, in the right-hand side, including municipality fixed effects; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure. See Appendix A for the definition of each variable. Outcomes are normalized such that negative values mean worse cognitive performance.

3. The figure displays the effects of the indicator variable of the survey being within 7 days *before* payday (relative to after) on cognitive load and tunneling, restricting attention to observations in a 7-day window around payday. The left-hand side panel showcases effect sizes of this indicator variable on cognitive load, and the right-hand side, on tunneling. Bars reflect 95% confidence intervals, and thicker dots reflect higher levels of aggregation of summary measures (in line with pre-registration).

Figure 4 – Non-parametric effects of distance to payday on cognitive load and tunneling

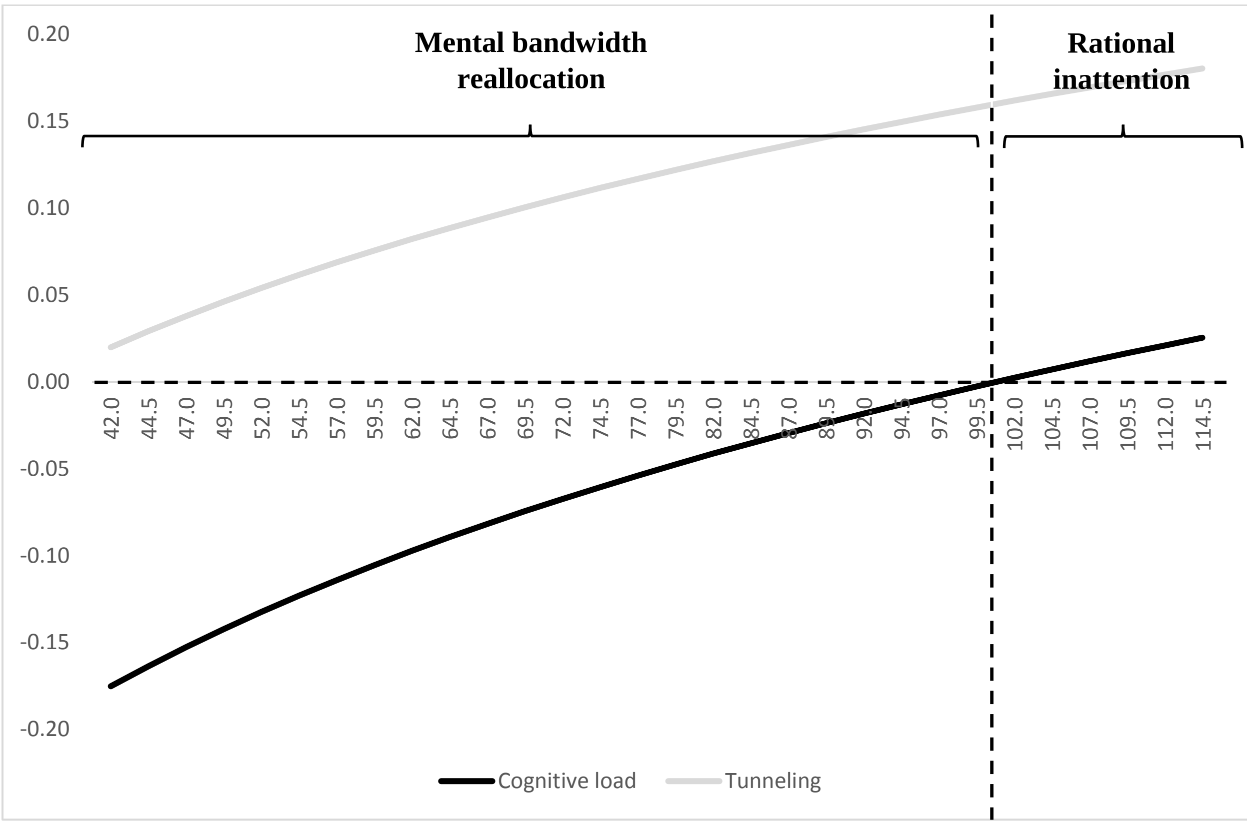


Notes on Figure 4:

1. The figure displays coefficients (dots) and 95% confidence intervals (bars) for the summary cognitive effects of being before (versus after) payday, in symmetric time windows around payday. Each (dot) estimate holds the window size fixed, only comparing subjects surveyed before vs. after payday within the time window specified on the X-axis. Regressions include municipality fixed effects;
2. All estimates are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for executive functions and anchoring, in the left-hand side, and for focus and framing, in the right-hand side, including municipality fixed effects; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure. See Appendix A for the definition of each variable. Outcomes are normalized such that negative values mean worse cognitive performance.

Figure 5 – Predicted effects on cognitive function by municipality's per capita income (monthly USD)

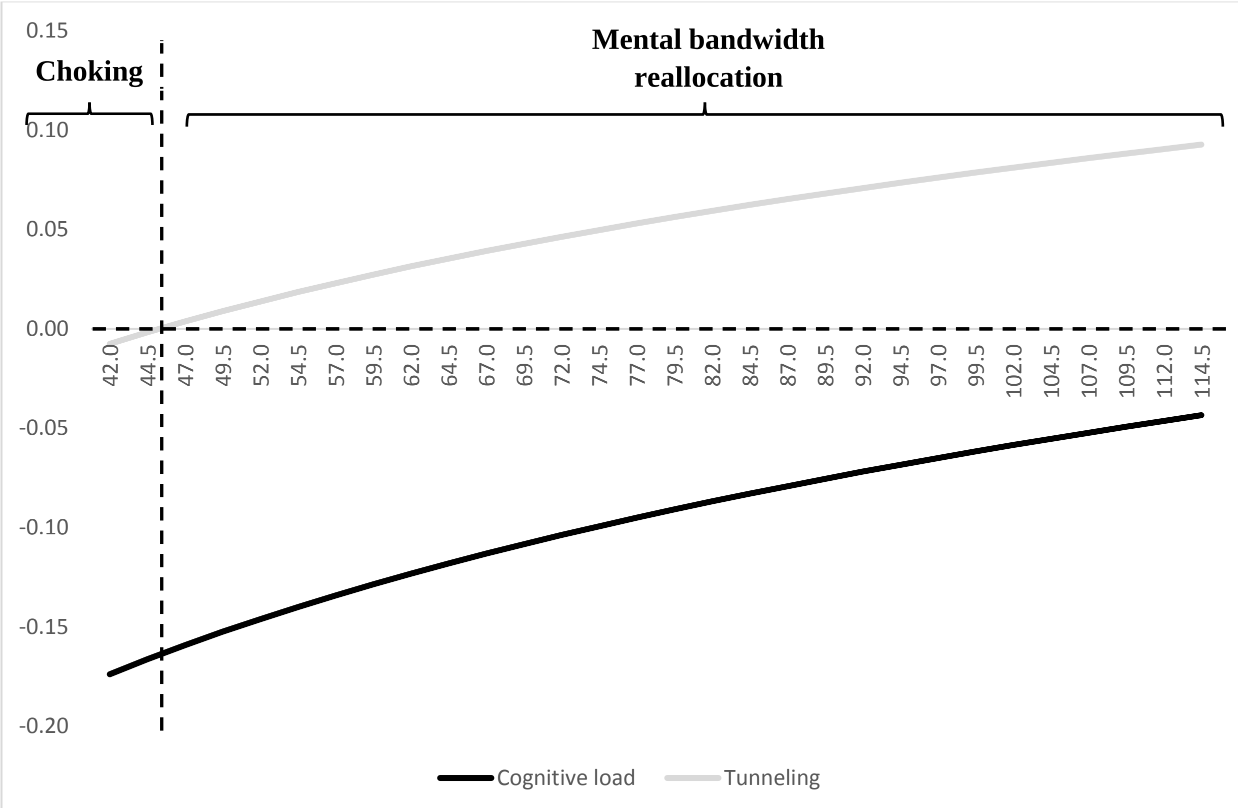
Panel A: Predicted effects of priming



Notes on Figure 5 – Panel A:

1. Predicted effects of priming on cognitive load and tunneling based on the estimates from Table 8, within the range of monthly per capita income of the municipalities in our sample;
2. We call *mental bandwidth reallocation* the simultaneous occurrence of cognitive and tunneling;
3. We call *rational inattention* the phenomenon that, among the subjects in the least poor municipalities, priming *only enhances* their performance in tasks involving scarce resources, without deteriorating their performance overall.

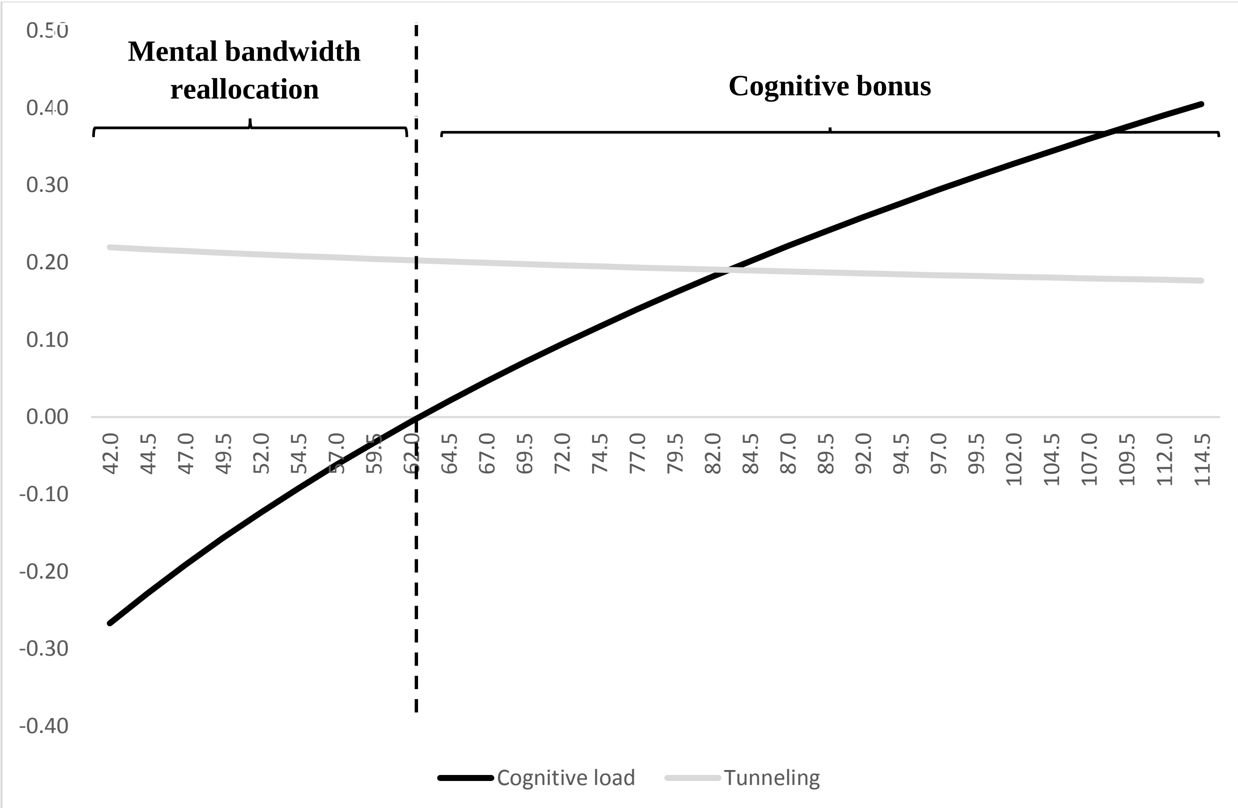
Panel B: No rainfall in t-3



<u>Notes on Figure 5 – Panel B</u>:

1. Predicted effects of no rainfall in t-3 on cognitive load and tunneling based on the estimates from Table 8, within the range of monthly per capita income of the municipalities in our sample;
2. We call *choking* the phenomenon that the negative impacts of scarcity on cognitive function among the subjects in the poorest municipalities are *magnified* – rather than (partially) reversed –, within tasks involving scarce resources, presumably a reaction to high stakes in line with Ariely et al. (2009);
3. We call *mental bandwidth reallocation* the simultaneous occurrence of cognitive and tunneling.

Panel C: Bolsa Família payment within next 7 days

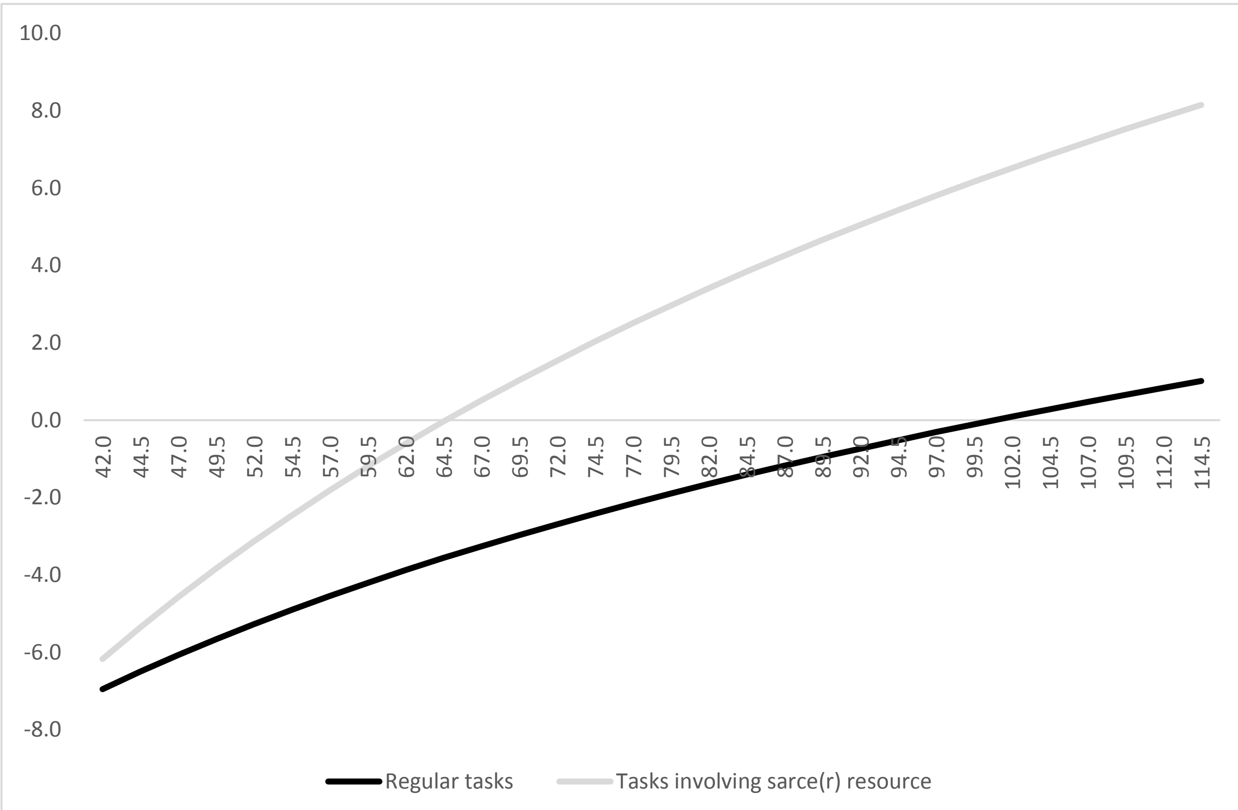


<u>Notes on Figure 5 – Panel C:</u>

1. Predicted effects of CCT payment within next 3 days on cognitive load and tunneling based on the estimates from Table 8, within the range of monthly per capita income of the municipalities in our sample;
2. We call *mental bandwidth reallocation* the simultaneous occurrence of cognitive and tunneling;
3. We call *cognitive bonus* the improvement in cognitive performance across all dimensions, particularly in tasks involving scarce resources.

Figure 6 – Predicted percentile changes of cognitive performance *by type of task*, by municipality's per capita income (monthly USD)

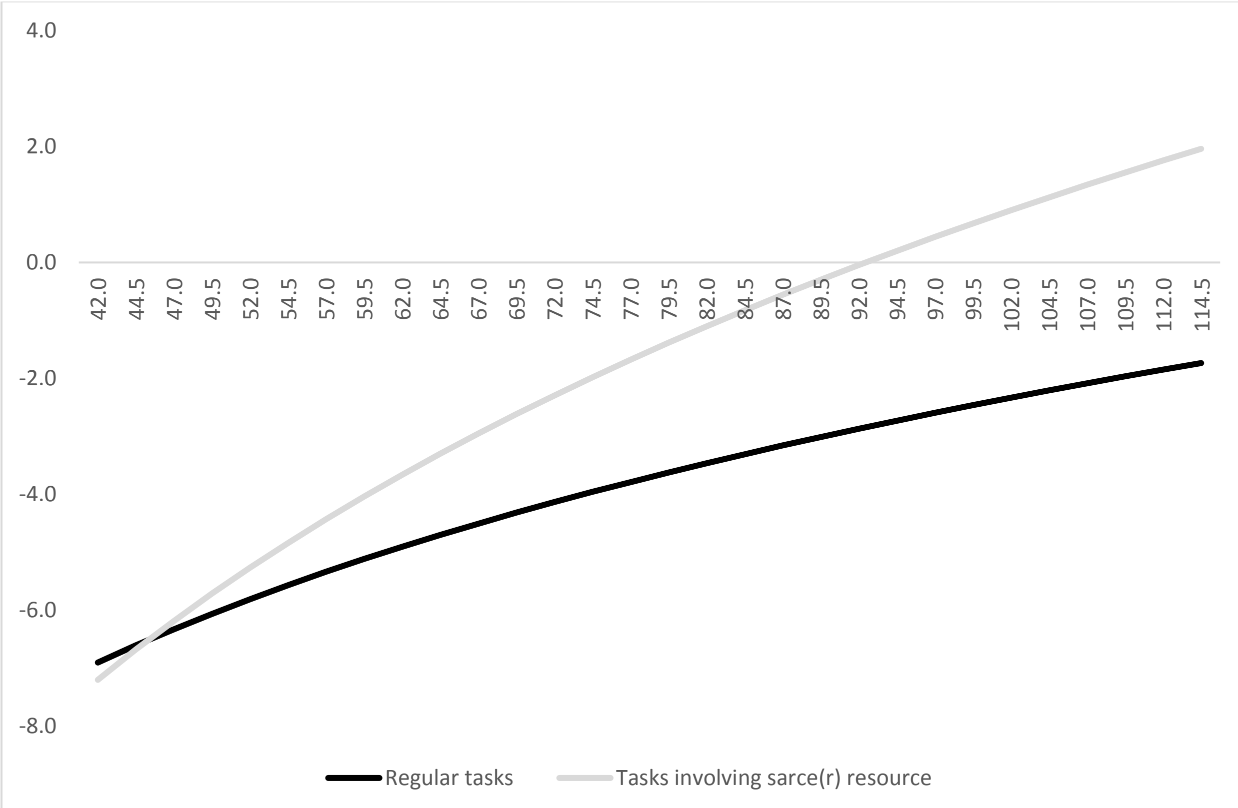
Panel A: Priming



<u>Notes on Figure 6 – Panel A:</u>

1. Percentile changes for *regular tasks* computed based on cognitive load effect sizes only, and those for tasks *involving scarce resources*, adding cognitive load and tunneling effect sizes;
2. Predicted effects of priming on cognitive load and tunneling based on the estimates from Table 8, within the range of monthly per capita income of the municipalities in our sample.

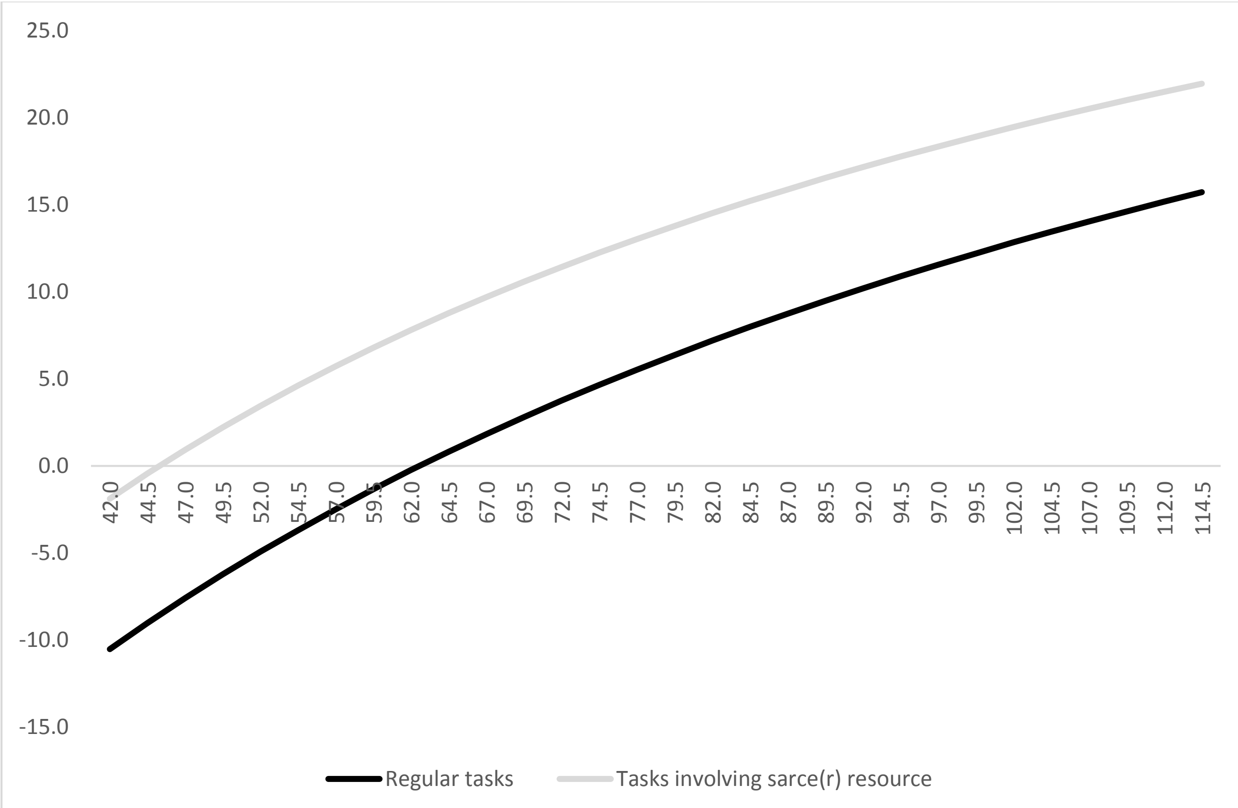
Panel B: No rainfall in t-3



<u>Notes on Figure 6 – Panel B:</u>

1. Percentile changes for *regular tasks* computed based on cognitive load effect sizes only, and those for tasks *involving scarce resources*, adding cognitive load and tunneling effect sizes;
2. Predicted effects of no rainfall in t-3 on cognitive load and tunneling based on the estimates from Table 8, within the range of monthly per capita income of the municipalities in our sample.

Panel C: Bolsa Família payment within 7 days



<u>Notes on Figure 6 – Panel C:</u>

1. Percentile changes for *regular tasks* computed based on cognitive load effect sizes only, and those for tasks *involving scarce resources*, adding cognitive load and tunneling effect sizes;
2. Predicted effects of Bolsa Família payment within 7 days on cognitive load and tunneling based on the estimates from Table 8, within the range of monthly per capita income of the municipalities in our sample.

Tables

Table 1 – Effects of priming, rainfall and distance to payday on worries about rainfall and household bills

	Full Sample				March-April		Full Sample	BF sub-sample
	Worries about rainfall				Worries rainfall	Worries HH bills	Worries rainfall	Worries rainfall
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Priming	0.050	0.057*	0.148***	0.003	0.114**	0.013	0.054	0.009
	(0.034)	(0.034)	(0.056)	(0.065)	(0.047)	(0.052)	(0.034)	(0.060)
Wave			0.106***					
			(0.021)					
Priming x Wave			-0.068**					
			(0.030)					
No-rainfall summary measure							0.221***	0.176***
							(0.048)	(0.061)
Priming x No rainfall s.m.							-0.098	
							(0.062)	
Distance to payday								-0.003
								(0.003)
Priming x				0.141				
Harvest loss (previous year)				(0.156)				
Municipality Fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Wave Fixed effects	N	Y	N	N	N	N	N	N
Observations	3,871	3,871	3,871	3,871	2,131	1,929	3,871	1,212
R-squared	0.031	0.043	0.038	0.031	0.047	0.040	0.038	0.065

Notes on Table 1:

1. All columns are OLS regressions with standardized worries (z-score) as dependent variable, about rainfall in columns (1)-(5) and (7)-(8), and about household bills in column (6). See Appendix A for the definition of each variable;
2. No-rainfall summary measure is a Post-LASSO measure of rainfall shocks predictive of worries about rainfall; see Table C2;
3. Robust standard errors in parenthesis, clustered at the individual level; *** p<0.01, ** p<0.05, * p<0.1.

Table 2 – Effects of priming and rainfall on cognitive load

	Cognitive Load					
	(1)	(2)	(3)	(4)	(5)	(6)
Priming	-0.045**		-0.050***			
	(0.020)		(0.020)			
No-rainfall summary measure		-0.108***	-0.113***			
		(0.020)	(0.0273)			
Priming x No rainfall s.m.			0.007			
			(0.036)			
No rainfall in t-3				-0.119***		
				(0.021)		
No rainfall in t-7					-0.097***	
					(0.021)	
Harvest loss						-0.173***
						(0.021)
Municipality Fixed effects	Y	Y	Y	Y	Y	N
Controls	Y	Y	Y	Y	Y	Y
Observations	2,362	2,362	2,362	2,362	2,362	490

Notes on Table 2:

1. All columns are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for executive functions and anchoring; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each component. See Appendix A for the definition of each variable;
2. Outcomes are normalized such that negative values mean worse cognitive performance (lower attention, memory and impulse control, and higher sensitivity to anchoring);
3. No-rainfall summary measure is a Post-LASSO measure of rainfall shocks predictive of worries about rainfall; see Table C2;
4. Controls include all baseline variables reported in Tables D1 to D3;
5. The number of observations reported is the minimum across all summary measure components;
6. Robust standard errors in parenthesis, clustered at the individual level; *** p<0.01, ** p<0.05, * p<0.1.

Table 3 – Effects of priming and rainfall on tunneling

	Tunneling					
	(1)	(2)	(3)	(4)	(5)	(6)
Priming	0.043**		0.0382*			
	(0.021)		(0.021)			
No-rainfall summary measure		0.043*	0.003			
		(0.024)	(0.031)			
Priming x No rainfall s.m.			0.081*			
			(0.042)			
No rainfall in t-3				0.048*		
				(0.027)		
No rainfall in t-7					-0.009	
					(0.028)	
Harvest loss						0.014
						(0.0318)
Municipality Fixed effects	Y	Y	Y	Y	Y	N
Controls	Y	Y	Y	Y	Y	Y
Observations	1,138	1,138	1,138	1,138	1,138	532

Notes on Table 3:

1. All columns are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\widehat{\beta}_j}{\widehat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for focus and framing; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each component. See Appendix A for the definition of each variable;
2. Outcomes are normalized such that positive values mean better relative cognitive performance (higher relative attention towards scarce resources, higher valuation of scarce resources, and lower sensitivity to framing biases in tasks involving scarce resources);
3. No-rainfall summary measure is a Post-LASSO measure of rainfall shocks predictive of worries about rainfall; see Table C2;
4. Controls include all baseline variables reported in Tables D1 to D3;
5. The number of observations reported is the minimum across all summary measure components;
6. Robust standard errors in parenthesis, clustered at the individual level; *** p<0.01, ** p<0.05, * p<0.1.

Table 4 – Lee Bounds for the effects of priming and rainfall on cognitive loads and tunneling

	Cognitive Load		Tunneling	
	Lower Bound (1)	Upper Bound (2)	Lower Bound (3)	Upper Bound (4)
Priming	-0.045	-0.043	0.043	0.043
IC 90% =	-0.077	-0.011	0.008	0.078
No Rainfall in t-3	-0.119	-0.114	0.047	0.048
IC 90% =	-0.154	-0.078	0.002	0.092
No Rainfall in t-7	-0.097	-0.102	-0.009	-0.009
IC 90% =	-0.131	-0.068	-0.055	0.037
Payment within next 3 days	0.076	0.076	0.181	0.126
IC 90% =	-0.016	0.168	0.007	0.300
Payment within next 7 days	0.056	0.022	0.220	0.177
IC 90% =	-0.039	0.118	0.099	0.294
Municipality Fixed effects	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Notes on Table 4:

1. Each row represents a different regression;
2. All cells are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for executive functions and anchoring, in columns (1) and (2), and for focus and framing, in columns (3) and (4); where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure. See Appendix A for the definition of each variable.
3. Outcomes are normalized such that negative values mean worse cognitive performance (lower attention, memory and impulse control, and higher sensitivity to anchoring; lower relative attention towards scarce resources, lower valuation of scarce resources, and higher sensitivity to framing biases in tasks involving scarce resources);
4. Bounds computed following Lee (2009)'s procedure;
5. Robust standard errors in parenthesis, clustered at the individual level; *** p<0.01, ** p<0.05, * p<0.1.

Table 5 – Effects of priming, rainfall and distance to payday on cognitive function within the Bolsa Família sub-sample

	Priming	No Rainfall summary measure	Distance to payday	Payment within next 3 days	Payment within next 7 days
	(1)	(2)	(3)	(4)	(5)
Panel A: Worries					
Worries rainfall	0.002	0.174***	-0.003	-0.054	-0.054
	(0.060)	(0.061)	(0.003)	(0.114)	(0.086)
Worries bills	-0.063	0.094*	0.001	0.004	0.001
	(0.068)	(0.052)	(0.003)	(0.163)	(0.088)
Municipality Fixed effects	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y
Observations	1,929	1,929	357	357	357
Panel B: Cognitive Function					
Cognitive load	-0.064*	-0.150***	0.003	0.082	0.054
	(0.036)	(0.036)	(0.002)	(0.069)	(0.067)
Tunneling	0.045	0.051	-0.002	0.224**	0.21**
	(0.039)	(0.041)	(0.002)	(0.113)	(0.081)
Municipality Fixed effects	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y
Observations	1,138	1,138	1,055	1,055	1,055

Notes on Table 5:

1. Each cell represents a different regression. In cols (1) and (2), all cells are OLS regressions with standardized worries (z-score) as dependent variable. In cols (3) and (4), all cells are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated

Regressions (SUR) with standardized dependent variables (z-scores) for executive functions and anchoring, in column (3), and for focus and framing, in column (4); where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure in each column. See Appendix A for the definition of each variable;

2. Outcomes are normalized such that negative values mean worse cognitive performance (lower attention, memory and impulse control, and higher sensitivity to anchoring; lower relative attention towards scarce resources, lower valuation of scarce resources, and higher sensitivity to framing biases in tasks involving scarce resources);
3. No-rainfall summary measure is a Post-LASSO measure of rainfall shocks predictive of worries about rainfall; see Table C2.
4. The number of observations reported is the minimum across all summary measure components;
5. Robust standard errors in parenthesis, clustered at the individual level; *** p<0.01, ** p<0.05, * p<0.1.

Table 6 – Effects of priming, rainfall and distance to payday on cognitive load and focus by municipality's per capita income

	Priming	No-rainfall summary measure	No rainfall in t-3	No rainfall in t-7	Distance to payday	Payment within next 3 days	Payment within next 7 days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Cognitive Load							
Treatment	-1.2**	-0.35	-0.84	-1.1*	0.091	0.48	-3.7**
	(0.549)	(0.494)	(0.601)	(0.593)	(0.061)	(1.622)	(1.828)
Treatment x ln(per capita income)	0.2**	0.042	0.13	0.19*	-0.016	-0.07	0.67**
	(0.099)	(0.089)	(0.108)	(0.107)	(0.011)	(0.294)	(0.33)
Municipality Fixed effects	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y
Observations	2,632	2,632	2,632	2,632	759	759	759
Panel B: Tunneling							
Treatment	-0.85	-0.39	-0.52	0.021	-0.0051	2.6	0.44
	(0.566)	(0.596)	(0.672)	(0.733)	(0.063)	(2.577)	(2.704)
Treatment x ln(per capita income)	0.16	0.079	0.1	-0.0075	0.00063	-0.44	-0.043
	(0.102)	(0.107)	(0.121)	(0.132)	(0.011)	(0.471)	(0.486)
Municipality Fixed effects	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y
Observations	1,138	1,138	1,138	1,138	357	357	357

Notes on Table 6:

1. Each cell is a different regression. All cells are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for executive functions and anchoring, in Panel A, and for

focus and framing, in Panel B; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure. See Appendix A for the definition of each variable. Outcomes are normalized such that negative values mean worse cognitive performance;

2. No-rainfall summary measure is a Post-LASSO measure of rainfall shocks predictive of worries about rainfall; see Table C2;
3. ln(per capita income) from the 2010 Census by the Brazilian Institute for Geography and Statistics (IBGE);
4. The number of observations reported is the minimum across all summary measure components;
5. Robust standard errors in parenthesis, clustered at the individual level; *** p<0.01, ** p<0.05, * p<0.1.

Appendix A – Definition of dependent variables

WORRIES

Worries about rainfall:

"How much did you and your family worry last week about how much it will rain in the next month? If not at all, press 0, if a little, press 1, if a lot, press 2"

Worries about household bills:

"Was your household able to cope with ordinary bills and daily consumer items last week? If your household had no difficulty in coping, press 0, if it had some difficulty, press 1, if it had a lot of difficulties, press 2"

COGNITIVE LOAD

- Executive Functions

Digit span:

"Please type the sequence of numbers as you hear it. 4 8 2 0 5 / 5 2 9 1 7 / 0 3 6 4 8 / 9 1 9 2 1"

Stroop:

"How many times is number '9' repeated in the following? 9 9 9 9 / 6 6 6 6 6/ 0 0 0 / 5 5 5 5"

- Anchoring:

Price of beans:

"Last year's average price per kg of a live goat was R$ 4 in Ceará. Which of the following price bands best characterizes what the selling prices of beans in May will be in your municipality? If between 3 and 3.40 reais, press 1; if between 3.40 and 3.80, press 2; if between 3.80 and 4.20, press 3; or, if above 4.20 reais, press 4"

Price of subway ticket:

"Last year's average price per kg of a live goat was R$ 4 in Ceará. Which of the following price bands best characterizes what the price of a subway ticket in São Paulo is? If between 3 and 3.40 reais, press 1; if between 3.40 and 3.80, press 2; if between 3.80 and 4.20, press 3; or, if above 4.20 reais, press 4"

TUNNELING

- Focus:

Word search (water):

"If you hear WATER or HUSBAND among the following scrambled words, please press 1 at the end of each set; otherwise press 0: ÁLCOOL ; ALTO ; ÁGUA ; ARCO / PAI ; FILHO ; ESPOSA ; IRMÃO / LAGO ; NUVEM ; CHUVA ; SECA / QUERIDO ; PALITO ; MARIDO ; FERIDO"

Word search (water) = score[water] – score[neutral]

Word search (money):

"If you hear MONEY or BROTHER among the following scrambled words, please press 1 at the end of each set; otherwise press 0: CHIQUEIRO ; DINHEIRO ; MARINHEIRO ; PINHEIRO / IRLANDA ; SERMÃO ; LIMÃO ; SALMÃO / CHEQUE ; CARTÃO ; BANCO ; DÍVIDA / MARIDO ; PRIMO ; IRMÃO ; ESPOSA"

Word search (money) = score[money] – score[neutral]

Trade-off oranges vs. cashews:

"How many oranges would you offer to trade in 2 kg of cashews? If less than 1 liter, press 1, if between 1 and 4 liters, press 2, if between 4 and 7 liters, press 3, if between 7 and 10 liters, press 4, or if more than 10 liters, press 5."

Trade-off money vs. cashews:

"How much money would you offer to trade in 2 kg of cashews? If less than 2 reais, press 1; if between 2 and 5 reais, press 2; if between 5 and 8 reais, press 3; if between 8 and 11 reais, press 4; or, if more than 11 reais, press 5."

Tunneling (money) = [Trade-off oranges vs. cashews] – [Trade-off money vs. cashews]

Trade-off water vs. cashews:

"How many liters of water would you offer to trade in 2 kg of cashews? If less than 1 liter, press 1; if between 1 and 4 liters, press 2; if between 4 and 7 liters, press 3; if between 7 and 10 liters, press 4; or, if more than 10 liters, press 5."

Tunneling (water) = [Trade-off oranges vs. cashews] – [Trade-off water vs. cashews]

- Framing:

Trade-off money vs. time – low value:

"Consider the following scenario: Let's imagine you walk into a store to buy batteries which costs R$ 10. The seller tells you there is a store 40 minutes away which sells the same batteries for R$ 5. If you would buy them for R$ 10 anyway, press 1; if you would rather go to the other store to buy them for R$ 5, press 2"

<u>Trade-off money vs. time – high value</u>:

"Consider the following scenario: Let's imagine you walk into a store to buy an iron which costs R$90. The seller tells you there is a store 40 minutes away which sells the same iron for R$40. If you would buy it for R$90 anyway, press 1; if you would rather go to the other store to buy it, press 2"

> <u>Sensitivity to framing (money)</u>: money[high] *vs.* money[low], for the subset of individuals either primed in all calls, or not primed in any calls involving these outcomes.

<u>Trade-off water vs. time – low amount</u>:

"Consider the following scenario: Let's imagine you walk downtown to get 1 gallon of water from a water truck. A neighbor tells you there is another municipality, which takes 30 extra minutes to reach (and 30 extra to come back), where you could get 2 gallons of water from a water truck. If you would get water in your own town, press 1; if you would get water from the other municipality, press 2."

<u>Trade-off water vs. time – high amount</u>:

"Consider the following scenario: Let's imagine you walk downtown to get 5 gallon of water from a water truck. A neighbor tells you there is another municipality, which tales 30 extra minutes to reach (and 30 extra to come back), where you could get 6 gallons of water from a water truck. If you would get water in your own town, press 1; if you would get water from the other municipality, press 2."

> <u>Sensitivity to framing (water)</u>: water[high] *vs.* water[low], for the subset of individuals either primed in all calls, or not primed in any calls involving these outcomes.

Appendix B – Priming: treatment and control messages

- Call #1:

Treatment: "Please tell us after the tone what you would do in case your municipality is faced with a drought this year."

Control: "Please tell us after the tone what you would do in case the next prime-time soap opera is not good."

- Call #2:

Treatment: "Please tell us to what extent you think your income this year will be determined by rainfall."

Control: "Please tell us to what extent you think your sleep time will be determined by what is on TV."

- Call #3:

Treatment: "Please tell us to what extent you have been following the rainfall forecast this year and tell us why."

Control: "Please tell us to what extent you have been following the prime-time soap opera this year and tell us why."

- Call #4:

Treatment: "Please tell us what do you think determines whether the rainy season in your municipality will be good."

Control: "Please tell us what do you think determines whether the next prime-time soap opera in your municipality will be good."

- Call #5:

Treatment: "Please tell us to what extent rainfall matters for farmers in Ceará."

Control: "Please tell us to what extent soap operas matter for farmers in Ceará."

- Call #6:

Treatment: "Please tell us what you think the impacts of a drought are on family farmers."

Control: "Please tell us what you think the impacts of soap operas are on viewers."

Appendix C – Description of datasets

Table C1 – Number and percentage of subjects per number of surveys completed

No. of Surveys	Subjects	%
1	300	10.6
2	268	9.5
3	225	8.0
4	188	6.7
5	150	5.3
6	167	5.9
7	131	4.6
8	113	4.0
9	115	4.1
10	101	3.6
11	100	3.5
12	105	3.7
13	88	3.1
14	87	3.1
15	93	3.3
16	82	2.9
17	83	2.9
18	65	2.3
19	57	2.0
20	55	1.9
21	52	1.8
22	48	1.7
23	80	2.8
24	69	2.4

Notes on Table C1:

1. Distribution of the number of surveys (calls) completed by the 2,822 subjects reached by at least one call.

Table C2 – List of rainfall variables picked by LASSO as predictors of worries about rainfall

- Rainfall level in t-2
- Rainfall occurrence in t-7
- Rainfall occurrence in t-3
- Accumulated rainfall in the past 21 days
- Number of days with occurrence of rainfall in the last 2 days
- Number of days with occurrence of rainfall in the last 5 days
- Number of days with occurrence of rainfall in the last 21 days
- Relative deviation from mean in t-7
- Accumulated absolute deviation in the past 21 days

<u>Notes on Table C2:</u>

Variables to which LASSO assigns non-zero weight, in a regression featuring worries about rainfall as dependent variable, and including 51 features of rainfall over the past 21 days as independent variables, together with municipality fixed effects.

Table C3 – Distribution of call dates and Bolsa Família payments

		Wave			
		March	April	May	June
Call	1	9-10	6-7	4-5	15-16
	2	11-12	8-9	6-7	17-18
	3	13-14	10-11	8-9	19-20
	4	16-17	13-14	11-12	22-23
	5	18-19	15-16	13-14	24-25
	6	20-21	17-18	15-16	26-27
		Wave			
		March	April	May	June
NIS's last digit	1	18	16	18	17
	2	19	17	19	18
	3	20	20	20	19
	4	23	22	21	22
	5	24	23	22	23
	6	25	24	25	24
	7	26	27	26	25
	8	27	28	27	26
	9	30	29	28	29
	0	31	30	29	30

Notes on Table C3:

1. Distribution of call dates and Bolsa Família paydays by survey wave.

Table C4 – Distribution of payday among Bolsa Família beneficiaires

		Frequency (%)			
Days until payday	All waves	March	April	May	June
-15	1.64	1.19	0	0.45	0
-14	5.37	1.65	0	1.31	2.4
-13	3.52	1.19	0	1	1.34
-12	3.09	0.82	0	0.85	1.42
-11	2.58	0.78	0	0.45	1.35
-10	2.19	0.8	0	0.41	0.98
-9	1.62	0.35	0.31	0	0.96
-8	1.59	0.33	0.36	0	0.9
-7	1.91	0.35	0.6	0	0.96
-6	1.59	0.33	0.36	0	0.9
-5	1.04	0	0.6	0	0.44
-4	1.12	0	0.72	0	0.39
-3	0.56	0	0.56	0	0
-2	1.49	0	1	0.49	0
-1	1.88$	0.37	0.92	0.59	0
0	3.02	0.37	1.65	1	0
1	1.88	0.37	0.92	0.59	0
2	2.65	0.37	1.29	1	0
3	3.67	0.74	1.27	1.19	0.48
4	2.97	0.37	1.15	0.92	0.53
5	4.3	0.85	0.96	1.55	0.94
6	4.17	0.8	0.78	1.55	1.04
7	6.76	1.57	1.31	2.5	1.38
8	4.17	0.8	0.78	1.55	1.04
9	4.7	1.21	0.69	1.9	0.9
10	5.77	1.59	0.54	2.18	1.45
11	5.03	1.19	0.58	1.77	1.49
12	5.32	1.63	0.26	1.51	1.92
13	4.83	1.23	0.23	1.37	2
14	6.94	1.98	0.26	1.95	2.75
15	2.6	0.46	0.23	1.37	0.53

Notes on Table C4:

1. Distribution of the number of surveys (calls) completed by the 2,822 subjects reached by at least one call.

Appendix D – Balance and selective non-response tests

Table D1 – Balance tests: Priming

	Priming = 0	Priming = 1	Difference [1 - 0]	Difference [1-0] with Mun. FE
Male	0.338	0.338	-5.99E-05	0.00195
	[0.0139]	[0.0139]	[0.0110]	[0.0109]
Age	35.54	35.18	-0.368	-0.366
	[0.622]	[0.608]	[0.372]	[0.424]
Believes in RoT	0.659	0.670	0.0115	0.00531
	[0.0143]	[0.0140]	[0.0112]	[0.0114]
Irrigation	0.138	0.134	-0.00321	-0.00333
	[0.0115]	[0.0112]	[0.00668]	[0.00718]
Owns property	0.318	0.316	-0.00174	0.00221
	[0.0165]	[0.0168]	[0.0133]	[0.0139]
Plot size	7.142	6.583	-0.559	-0.148
	[1.193]	[0.944]	[0.472]	[0.409]
Cassava	0.208	0.216	0.00794	0.00791
	[0.0139]	[0.0144]	[0.00789]	[0.00754]
Number of rooms	5.200	5.122	-0.0778**	-0.0797**
	[0.0545]	[0.0551]	[0.0337]	[0.0332]
Household income	1.657	1.651	-0.0062	0.000677
	[0.0262]	[0.0261]	[0.0148]	[0.0153]
Schooling	2.158	2.127	-0.0313*	-0.0294*
	[0.0292]	[0.0296]	[0.0161]	[0.0177]
Bolsa Família	0.769	0.782	0.013	0.012
	[0.0153]	[0.0150]	[0.00863]	[0.00914]
Government insurance	0.795	0.789	-0.00655	-0.00749
	[0.0110]	[0.0113]	[0.00763]	[0.00796]

Notes on Table D1:

1. Columns (1) and (2) present the averages for each variable collected at baseline (February) for the control group and for the treatment group, respectively;
2. Column (3) presents the unconditional difference between the treatment and control groups for each variable collected at baseline, and column (4) presents the within municipality-survey difference between the treatment and control groups for each variable collected at baseline;
3. Averages and p-values computed from the sample of all individuals with information for baseline characteristics, and treatment assignment across all 24 surveys.

Table D2 – Balance tests: Rainfall shocks

Panel A: No rainfall in t-3

	No Rainfall in t-3 = 1	No Rainfall in t-3 = 0	Difference [1-0] with Mun. FE	Observations
Male	0.337	0.345	0.003	15192
	(0.017)	(0.012)	0.004	
Age	34.224	34.616	0.174	7674
	(0.675)	(0.544)	0.139	
Believes in RoT	0.66	0.663	0.01***	14550
	(0.018)	(0.013)	0.004	
Irrigation	0.135	0.139	0	21084
	(0.013)	(0.011)	0.003	
Owns Property	2.031	2.036	0.008	20460
	(0.032)	(0.025)	0.007	
Plot Size	5.115	6.655	0.24	2154
	(1.182)	(0.858)	0.167	
Cassava	0.171	0.2	0.002	19968
	(0.015)	(0.012)	0.003	
Number of Rooms	5.563	5.475	0.022	15042
	(0.081)	(0.065)	0.018	
Household Income	1.649	1.662	-0.003	18426
	(0.031)	(0.025)	0.006	
Schooling	2.106	2.143	-0.012*	17376
	(0.034)	(0.027)	0.007	
Bolsa Família	0.798	0.781	0.003	18234
	(0.017)	(0.014)	0.004	
Government Insurance	0.812	0.794	0.015**	21210
	(0.015)	(0.01)	0.007	
Municipality Fixed Effects	N	N	Y	
Wave Fixed Effects	N	N	N	

Panel B: No rainfall in t-7

	No Rainfall in t-7 = 1	No Rainfall in t-7 = 0	Difference [1-0] with Mun. FE	Observations
Male	0.319	0.338	-0.001	15192
	(0.019)	(0.013)	0.003	
Age	34.719	34.742	0.272*	7674
	(0.716)	(0.55)	0.144	
Believes in RoT	0.669	0.666	0.003	14550
	(0.02)	(0.013)	0.003	
Irrigation	0.121	0.135	-0.002	21084
	(0.014)	(0.01)	0.003	
Owns Property	2.042	2.039	0.014**	20460
	(0.033)	(0.026)	0.007	
Plot Size	4.821	6.571	0.289	2154
	(1.278)	(0.797)	0.217	
Cassava	0.141	0.191	-0.001	19968
	(0.016)	(0.012)	0.004	
Number of Rooms	5.494	5.459	0.003	15042
	(0.088)	(0.064)	0.028	
Household Income	1.642	1.66	-0.007	18426
	(0.032)	(0.025)	0.007	
Schooling	2.103	2.142	-0.009	17376
	(0.036)	(0.027)	0.008	
Bolsa Família	0.808	0.784	0.004	18234
	(0.017)	(0.014)	0.004	
Government Insurance	0.818	0.796	0.011	21210
	(0.016)	(0.011)	0.007	
Municipality Fixed Effects	N	N	Y	
Wave Fixed Effects	N	N	N	

Notes on Table D2:

1. Columns (1) and (2) present the averages for each variable collected at baseline (February) for the control group and for the treatment group, respectively. Column (3) presents the unconditional difference between the treatment and control groups for each variable collected at baseline, and column (4) presents the within municipality difference between the treatment and control groups for each variable collected at baseline;
2. Weighted averages and p-values computed from the sample of all individuals with information for baseline characteristics, and treatment assignment across all 24 surveys.

Table D3 – Balance tests: Payday variation

Panel A: Payment within 3 days

	Payment within 3 days = 1	Payment within 3 days = 0	Difference [1-0] with Mun. FE	Observations
Phone survey covariates (for matched observations)				
Male	0.359	0.347	0.021	5910
	(0.043)	(0.019)	0.037	
Age	36.231	36.719	-0.603	2556
	(1.169)	(0.926)	0.807	
Believes in RoT	0.722	0.654	0.06*	5628
	(0.041)	(0.02)	0.036	
Irrigation	0.162	0.17	0.002	8220
	(0.025)	(0.018)	0.016	
Owns Property	2.135	2.06	0.067**	8070
	(0.057)	(0.041)	0.034	
Plot Size	6.904	7.62	-0.208	882
	(1.775)	(1.622)	0.374	
Cassava	0.218	0.218	0.016	7890
	(0.029)	(0.021)	0.016	
Number of Rooms	5.55	5.441	0.105	5042
	(0.152)	(0.064)	0.107	
Household Income	1.755	1.695	0.028	7332
	(0.054)	(0.038)	0.032	
Schooling	2.128	2.091	0.028	7002
	(0.055)	(0.041)	0.033	
Government Insurance	0.764	0.768	-0.015	8394
	(0.026)	(0.017)	0.018	
CadÚnico covariates (matched or unmatched)				
Male	0.833	0.814	0.013	15060
	(0.019)	(0.013)	0.014	
Age	0.528	0.473	0.048***	14286
	(0.027)	(0.017)	0.017	
Household Income	0.35	0.353	0.007	9648
	(0.03)	(0.02)	0.015	
Number of Bedrooms	46.479	46.366	0.23	15066
	(0.497)	(0.335)	0.355	
Read or Write	0.261	0.272	-0.008	15066
	(0.023)	(0.015)	0.017	

Worked in the last 12 months	90.348	90.412	-3.79	15066
	(7.102)	(4.983)	5.116	
Family Agriculture	2.089	2.078	0.021	14352
	(0.046)	(0.029)	0.032	
Municipality Fixed Effects	N	N	Y	
Wave Fixed Effects	N	N	N	

Panel B: Payment within 7 days

	Payment within 7 days = 1	Payment within 7 days = 0	Difference [1-0] with Mun. FE	Observations
Phone survey covariates (for matched observations)				
Male	0.359	0.347	0.021	5910
	(0.043)	(0.019)	0.037	
Age	36.082	36.744	-0.659	2556
	(1.232)	(0.93)	0.908	
Believes in RoT	0.722	0.654	0.06*	5628
	(0.041)	(0.02)	0.036	
Irrigation	0.156	0.171	-0.007	8220
	(0.026)	(0.018)	0.018	
Owns Property	2.153	2.056	0.093**	8070
	(0.06)	(0.041)	0.038	
Plot Size	6.705	7.663	-0.11	882
	(1.764)	(1.655)	0.475	
Cassava	0.215	0.218	0.008	7890
	(0.03)	(0.021)	0.019	
Number of Rooms	5.503	5.442	0.072	5042
	(0.12)	(0.064)	0.081	
Household Income	1.718	1.696	-0.004	7332
	(0.058)	(0.038)	0.037	
Schooling	2.126	2.09	0.037	7002
	(0.054)	(0.041)	0.035	
Government Insurance	0.764	0.768	-0.015	8394
	(0.028)	(0.017)	0.019	

CadUnico covariates (matched or unmatched)				
Male	0.83	0.813	0.013	15060
	(0.021)	(0.013)	0.016	
Age	0.522	0.472	0.047***	14286
	(0.029)	(0.017)	0.018	
Household Income	0.355	0.353	0.01	9648
	(0.032)	(0.02)	0.017	
Number of Bedrooms	46.714	46.348	0.486	15066
	(0.517)	(0.336)	0.396	
Read or Write	0.259	0.273	-0.014	15066
	(0.024)	(0.015)	0.018	
Worked in the last 12 months	86.499	90.668	-6.574	15066
	(6.973)	(5.024)	5.487	
Family Agriculture	2.065	2.079	0	14352
	(0.048)	(0.029)	0.036	
Municipality Fixed Effects	N	N	Y	
Wave Fixed Effects	N	N	N	

Notes on Table D3:

1. Columns (1) and (2) present the averages for each variable collected at baseline (February) for the control group and for the treatment group, respectively. Column (3) presents the unconditional difference between the treatment and control groups for each variable collected at baseline, and column (4) presents the within municipality difference between the treatment and control groups for each variable collected at baseline;
2. Weighted averages and p-values computed from the sample of all individuals with information for baseline characteristics, and treatment assignment across all 24 surveys.

Table D4 – Selective non-response tests

	Complete call
Panel A: Full Sample	
Priming	0.000049
	(0.006)
No-rainfall summary measure	0.013*
	(0.006)
No rainfall in t-3	0.012*
	(0.007)
No rainfall in t-7	-0.0011
	(0.006)
Panel B: Bolsa Família Sample	
Priming	-0.013
	(0.011)
No-rainfall summary measure	0.021*
	(0.012)
No rainfall in t-3	0.017
	(0.012)
No rainfall in t-7	0.0049
	(0.011)
Distance to payday	-0.00057
	(0.001)
Payment within 3 days	0.027
	(0.03)
Payment within 7 days	0.022
	(0.022)

Notes on Table D4:

1. Each cell is a different Ordinary Least Squares (OLS) regression, with dependent variable equal to 1 if the survey (call) was completed by the subject, and 0 otherwise, including municipality fixed effects;
2. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table D5 – Marginal effects of baseline characteristics on the probability of completing a call

Variable	Marginal effect on probability of completing a call
Respondent lives in municipality's most drought-prone region	0.02**
Respondent is male	-0.01
Respondent's age	-0.00**
Respondent believes that rainy season will be good if it rains on March 19th	0.02
Respondent's plot is at least partly irrigated	-0.05***
Respondent owns their property	-0.01
Respondent seeds cassava	0.00
Number of rooms in respondent's household	0.00
Respondent's average household income	-0.01
Respondent's schooling	0.02**
Respondent's household is a beneficiary of *Bolsa Família*	0.02
Respondent enrolled in Government insurance (*Garantia Safra*)	-0.02*

Notes on Table D5:

1. Cells are coefficients from an Ordinary Least Squares (OLS) regression, with an indicator variable as dependent variable, equal to 1 if the survey (call) was completed by the subject, and 0 otherwise, including municipality fixed effects;
2. *** p<0.01, ** p<0.05, * p<0.1.